THE PRACTITIONER.

THE PRACTITIONER:

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OF

THERAPEUTICS AND PUBLIC HEALTH.

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THE PRACTITIONER.

JANUARY, 1883.

Original Communications.

ON SOME POINTS IN RELATION TO THE SOUNDS OF THE HEART.[1]

BY W. H. BROADBENT, M.D., F.R.C.P.

THE time has long gone by when the recognition of a murmur at the base or apex, systolic or diastolic in time, or even of obstruction or regurgitation at one or other of the orifices or valves, could be accepted as an adequate diagnosis of heart-disease. If a valvular lesion exists, the degree of obstruction or amount of regurgitation must be estimated, the condition of the walls and cavities of the heart must be ascertained, and the effects of the interference with the current of blood must be traced.

But in a large proportion of affections of the heart, and these some of the most deadly, there are no murmurs from first to last, and we have to seek the elements of a diagnosis in other evidences of structural change or functional failure—in the seat and character of the apex-beat, in the situation, extent, force, and character of the impulse elsewhere than at the apex, in the dimensions of the heart and the relative size of its different chambers as revealed by percussion, and in modifications of the sounds. A careful study of the sounds of the heart is

[1] This Paper was read before the Harveian Society on November 2, 1882.

even more fruitful of information than exact discrimination between murmurs and careful estimations of their significance.

Before proceeding to the consideration of the special points which will form the principal subject of this communication, I must enumerate the principal modifications of the sounds which are met with, and indicate briefly their significance, as it is by the combination of all the deviations from the normal character of the sounds that a complete knowledge of the conditions of the heart which they serve to reveal is obtained. It is usual to speak of two sounds of the heart; but there are of course, four, a left-ventricle first sound and aortic second, and a right-ventricle first and pulmonary second, though, as a rule, the two ventricular and the two arterial sounds are exactly synchronous, and are consequently heard as one. It would be useless to talk of modifications of the sounds, however, unless we distinguished between those of the two sides of the heart: this is done chiefly by listening at certain points in the cardiac area.

The left ventricle first sound is best heard, uncomplicated by the first sound of the right ventricle, at and a little to the left of the apex; its distinctive character is, that as compared with the short and sharp second sound it is dull and long. The modifications which it may undergo are variations in intensity or loudness, and changes in character and duration. It becomes louder whenever the heart is beating more forcibly than usual, as during or just after exertion or excitement, in palpitation, in sthenic pyrexia, &c., but increased intensity rarely becomes a sign indicative of disease of the heart itself. When from resistance to the onward movement of the blood at the periphery or from obstruction at the aortic orifice, the ventricular contraction is more powerful than in the natural state of the circulation, the first sound does not necessarily become louder, it may, indeed, when there is compensatory hypertrophy, be less distinct from being prolonged. Another point, besides the neighbourhood of the apex, at which the left ventricle first sound may be heard without interference by the first sound of the right ventricle, is over the aortic area, *i.e.* in the right second intercostal space; and here it often becomes altogether inaudible when there is high-pressure in the arterial system and consequently a more vigorous contraction of the left ventricle.

Weakness of the first sound may be of great clinical significance. In enteric fever it is one of the important points on which the prognosis turns: every one knows that when the first sound becomes short and feeble or ceases to be audible there is considerable danger. It may aid in the diagnosis of fatty degeneration of the heart, but in regard of this disease, it is not a factor to which cardinal importance can be attached, since the first sound may be quite inaudible in mere functional debility of the heart.

More important than variations in loudness or intensity are variations in the duration and character of the left ventricular sound. It is somewhat prolonged and more dull when the ventricle is hypertrophied, but very commonly what appears to be a prolonged first sound in the region of the apex is found on examination to be the result of duplication.

It is the shortening of the first sound of the left ventricle which is most significant of important change in the heart. As it becomes short it often grows louder, or at any rate more distinct, and the pitch may be at the same time raised so that it comes to resemble the second sound, and when no impulse can be felt it is sometimes difficult to distinguish between them.

It is in dilatation of the left ventricle that the short first sound is most common. When it is at the same time loud and low in pitch, the dilatation may be considered to be accompanied by some degree of compensatory hypertrophy; it may still be loud, but it will be sharper when the wall of the ventricle is thin; when the dilatation is associated with weakness of the heart's action, whether from degeneration or other cause, the short first sound will also be weak. All these modifications may be followed in a case in which from renal disease or other condition there is high arterial tension, as the heart first meets and overcomes, and later yields, to the increased resistance. Corroborative evidence will be furnished by changes in the seat and character of the apex-beat and impulse, and by changes in the size of the heart as ascertained by deep percussion.

My own experience of fatty degeneration of the heart would lead me to conclude that in this condition the first sound is peculiarly short and sharp but weak, and that there is no true

impulse or push, but only what may be called a tap against the chest wall. It is stated however that the sound may be quite normal and the impulse good up to the moment of death. Possibly the explanation may be that in these cases the degeneration is partial, affecting only a part of the ventricular wall.

In mitral stenosis the first sound as heard at and near the apex becomes extremely short and sharp, and when the presystolic murmur is lost, as is often the case as the disease advances, this modification of the sound may be the main element in the diagnosis. I am not sure, however, that it is the first sound of the left ventricle in this case.

The first sound of the right ventricle is most distinct a little to the left of the lower end of the sternum, but it is heard over almost the whole cardiac area. It is always shorter and usually louder than the first sound of the left ventricle, notwithstanding the fact that pressure in the pulmonary circulation is lower than in the systemic, and therefore the force of the systole of the right ventricle is less than that of the left. This is probably due to the close contact of the right side of the heart with the chest-wall, but perhaps in some degree also to the thin wall of the right ventricle, which will lend itself better either to the production of sound or to its convection than a thicker mass of muscular substance. The right ventricle first sound is, like that of the left, intensified whenever the heart is acting with unusual energy, but it becomes disproportionately loud when the pressure in the pulmonary circulation is raised by resistance in the capillaries of the lungs as in bronchitis, emphysema, &c., or by disease on the left side of the heart. Very frequently forcible action of the right ventricle is accompanied by a curious clicking sound superimposed upon the intensified normal sound, best heard over the cartilages of the lower ribs.

The first sound of the right ventricle may survive that of the left when this is lost, though it may be itself much enfeebled. Sometimes, on the other hand, it becomes extinct while that of the left persists. I have only met with very marked enfeeblement or absolute loss of this sound in elderly people with failing heart. I have one case under observation in which for more than two years there has been no right ventricle

impulse, and neither right ventricle first nor pulmonary second sound. The patient has repeatedly been in great danger, and that he has not long since died was quite incomprehensible to me, until I was reminded, by Dr. MacAlister's recent instructive lecture on the form and mechanism of the heart, published in the *British Medical Journal*, October 28th, how greatly the left ventricle contributes to the passage of the blood through the right cavities.

Under certain conditions, chiefly when there is hypertrophy and dilatation of the left ventricle as a result of renal disease, or on the other hand, when the right ventricle is overtaxed by resistance in the pulmonary circulation, the two ventricular sounds are not absolutely synchronous. When this is the case the first sound as heard either over the right ventricle or just outside the apex is prolonged and murmurish, as it was termed by the late Dr. Sibson, but when the stethoscope is planted a little to the inner side of the apex, the confused sound resolves itself into two distinct elements, and is found to be duplex or reduplicated. That is, the right and left first sounds are heard separately, and the reason why they are best so heard at the point mentioned is that here the stethoscope is over the septum between the ventricles and upon a small part of each, so that it conveys the sound from both. I am aware that this interpretation of reduplication of the first sound which is Dr. Sibson's has been disputed by Dr. Johnson, who says that the first element is the sound of the auricular systole, and questioned by Prof. D'Espine of Geneva, who adduces strong evidence to show that the ventricular systole is *à deux temps*, but it is the one to which I still unhesitatingly adhere.

The aortic second sound is heard in the second right intercostal space close to the edge of the sternum, and from this spot upwards to the right sterno-clavicular articulation; it is also audible over the carotids, especially the right. It is again heard at, and to the left of, the apex, and sometimes peculiarities of this sound are more clearly recognised here than over the so-called aortic area proper, a fact the importance of which has not been fully recognised. A very loud pulmonary second sound heard over the entire right ventricle loses its intensity the instant the interventricular septum is crossed near the apex;

and conversely over the small portion of the left ventricle, which here approaches the surface, any peculiarity of the aortic second sound heard in the aortic area will be found to be repeated, perhaps even intensified. It is a disputed question whether the aortic or the pulmonary second sound is the louder; sometimes it is one, sometimes the other, and we cannot therefore make use of comparison between them in estimating variations in the intensity of either. There are such variations of intensity, however, and they are of great importance. A weak aortic second sound, not the result of valvular disease, is evidence of a weak ventricular contraction from some cause or other. An intensified or accentuated aortic second sound may have a far-reaching significance, and it is extremely common. In forming a judgment whether an aortic second sound is unduly loud or not, it may be compared with our average experience, or with the pulmonary second sound, or with the left ventricle first sound; we must really take all three comparisons into the estimate, and most important aid is obtained by listening to the sound at, and to the left of, the apex. Accentuation may be the result of forcible or of sudden contraction of the ventricle, but it is, as commonly met with, for the most part the effect of resistance in the peripheral circulation and consequent high arterial tension. A further modification of the aortic second sound—a lowered pitch and a ringing tone—is frequently confounded with accentuation, but it is really something quite distinct, usually superimposed upon accentuation, but sometimes independent of it. It is indicative of dilatation or aneurism of the aorta.

Accentuation of the pulmonary second sound may, as in the case of the aortic, be produced by forcible or sudden contraction of the corresponding ventricle, but it is usually the effect of increased tension in the pulmonary circulation, due sometimes to obstruction within the lungs from disease of these organs; sometimes to obstruction arising from disease in the left side of the heart; sometimes apparently from conditions of blood which disturb the normal relation between the blood and the pulmonary tissues and induce resistance in the pulmonary capillaries, just as resistance is set up in the systemic capillaries in renal disease.

Reduplication of the second sound from precipitate or retarded closure of pulmonary or aortic semilunar valves disturbing the normal synchronism between the two is a frequent effect of mitral stenosis and of pericarditis, and it may be met with as a result of adherent pericardium, and of other structural or valvular lesions. It is rarely produced by the systemic resistance and high arterial tension which give rise to reduplication of the first sound; more frequently by high tension in the pulmonary circulation due to disease of the lungs. It is in effect the pulmonary second sound which is displaced, as is perhaps sufficiently shown by the fact that reduplication can be induced simply by holding the breath. Sometimes cerebral disease is attended with marked reduplication of the second sound of the heart. It only remains to be said about this reduplication that it may be heard either at the base or at the apex, occasionally over a considerable part of the right ventricle; at the base, when the stethoscope is so placed as to be partly over the aortic, partly over the pulmonary area, or when it rests over the conus arteriosus, if the aortic sound is sufficiently loud to be carried through the shallow stratum of blood contained in it; at the apex, when the stethoscope is over the interventicular septum so as to receive the second sound conducted along the two ventricles: this septum being sometimes however in mitral stenosis carried up to or beyond the apex by dilatation and hypertrophy of the right ventricle, the left not enlarging.

We come now to the modifications in the sounds of the heart, which form the main subject of this communication, attention to which adds very much to the significance of those already enumerated; these are changes in the rhythm of the sounds. In the normal condition the interval between the first and second sounds is shorter than that between the second and the recurrence of the first—there is a short first pause and a longer second. The difference is such that the ordinary time-relation between the sounds may almost be represented by the triple time of musical notation; one, two, representing the two sounds, and a silent three the long pause; the accent will be on the 1 or 2 according to the comparative loudness of the first or second sound, or, to the point at which the stethoscope is

applied—1′, 2, ³, or 1, 2′, ³, as the case may be. When the pulse is exceptionally slow, although the first pause is prolonged, the second is prolonged in a greater degree, and the rhythm of the sounds will be represented by common musical time, 1, 2, 3, 4, of which 3 and 4 are silent. Increased frequency of the heart's action from physiological causes disturbs the relation between the intervals to some extent, but the diminution in the time occupied by a complete cardiac revolution is not entirely at the expense of diastole; the systole is more rapid, and the interval between the first and second sounds is shortened, as well as that between the second and first. We cannot, however, when the pulse is frequent, count 1, 2, ³, without hurrying over the ³ a little.

The period between the first and second sounds is occupied by the ventricular systole, and under normal conditions corresponds no doubt exactly with it in duration. I have therefore read with much interest the paper by Dr. P. M. Chapman on the duration of the ventricular systole, published in the *British Medical Journal* of August 19th, in which the exact relation between the time of systole and diastole is determined at different pulse-rates, from 50 to 130 per minute, both in a large number of individuals (90) and in the same individual at rest, and when the pulse has been made frequent by exercise, emotion, or the Turkish bath. His results confirm my own less exact and less reliable conclusions.[1] The late Dr. A. H. Garrod attempted, in 1871, to solve the same problem as Dr. Chapman, and formulated the law that the duration of the ventricular systole varied inversely as the square root of the rapidity of the heart's action. This law I was quite certain at that time did not obtain in disease, and Dr. Chapman has shown that it is not true even for physiological variations in frequence, and it has seemed to me that in pyrexial rapidity of pulse the systole is shorter than when the same pulse-rate is induced by exertion, on account, no doubt, of the relaxation of the peripheral vessels. On the other hand, in

[1] During the summer Mr. Walter Pearce, formerly one of my clinical clerks, was endeavouring, with the aid of his brother, to make the heart-sounds record a graphic trace; but the end of the session arrived when they were apparently on the verge of success but before it was actually attained. I hope, however, that this will yet be effected.

the nervous excitement of sensitive women with rapid action of the heart the sounds are often equidistant. One of the phenomena of such excitement, which is witnessed every day in the consulting room, is a remarkable tightening up of the artery at the wrist, and the result of the general arterial contraction which this indicates is an increased resistance in the circulation and a consequent prolongation of the systole. If the cardiograph could be employed to record the duration of systole and diastole in disease, the extension of Dr. Chapman's investigation to morbid conditions of the heart would be of extreme interest, but in the cases in which departure from the normal rhythm is most marked there is often no cardiac impulse whatever. We have therefore only the sounds upon which to rely.

The most frequent modification of the time-relation between the two sounds is a prolongation of the interval between the first and second, indicating a greater duration of the systole. This is very common as a result of high arterial tension, when it may be associated with the prolonged first sound of hypertrophy. While, however, the hypertrophy is such as to compensate for the increased resistance in the peripheral circulation, the lengthening of the systolic interval is not so considerable as to excite notice. It is when compensation is failing, and when dilatation is the predominant change present in the ventricle, that the second sound is most strikingly delayed, and the increased duration of the systole is all the more conspicuous from the fact that in this condition of the heart the first sound is usually short and sharp. It is not uncommon under these circumstances for the sounds to become actually equidistant, and when at the same time the first sound has come to resemble the second in character, it may be difficult to say which is first and which is second. The similar and equidistant sounds remind one of the ticking of a watch, or the tic-tac of a short pendulum, and I am in the habit of calling this modification by the name tic-tac. At times when the heart's action is frequent and these tic-tac sounds are very feeble they are not unlike the sounds of the fœtal heart.

The combination of a short first sound with a prolongation of the interval between this sound and the second, causing a

departure from the normal triple time and an approach to the tic-tac rhythm, is very frequently met with in renal disease. In nearly all the diseases of the kidney there is high pressure in the arteries, attributable in all probability to retained nitrogenised waste, which gives rise to resistance in the capillary circulation, and this throws upon the heart great increase of work; a more powerful contraction of the left ventricle is needed to force the blood through the capillaries. When the resistance increases very gradually, as in contracted granular kidney, hypertrophy of the heart usually proceeds *pari passu* with it, and for a time copes with the difficulty, but when the nutrition of the heart begins to fail with advancing years, or when the kidney disease affects an individual who is badly nourished or overworked, or who is debilitated by excesses, or privations, or disease, the ventricle yields and becomes dilated. With the dilatation come the modified first sound and prolonged first pause, which, carried to an extreme degree, give the tic-tac sounds. The dilatation may be rapid even in contracted granular disease of the kidney when there is antecedent degeneration of the walls of the heart or great general debility, but for the most part the modification of the sounds passes through the successive stages of prolonged first sound and reduplicated first sound, accentuation of the aortic second sound accompanying both, before the condition under consideration is reached.

In acute tubular or glomerular nephritis in which the capillary resistance is suddenly augmented while the muscular walls of the heart are enfeebled by pyrexia, the tic-tac character of the sounds often supervenes early, and when it lasts long it becomes, with associated absence of high tension in the pulse, a most unfavourable prognostic indication. Not unfrequently in this disease tic-tac sounds and a weak pulse in the early period are followed by a sustained pulse and reduplicated first sound, and these again by a normal condition of heart-sounds and pulse as the patient improves and finally recovers, these changes furnishing earlier information of favourable progress than even the urine.

High arterial tension from other causes than renal disease may produce the same effects on the heart, and the short first sound

and retarded second sound are very common in advanced life when the heart is beginning to fail.

According to my own experience the tic-tac character of the sounds is usually marked in fatty degeneration of the heart, but it does not establish the diagnosis of this condition ; and I have met with cases in which the rhythm has been preserved, though the sounds were extremely feeble, the clinical history and mode of death being suggestive of fatty degeneration. I have not, however, had post-mortem verification of the diagnosis under such circumstances.

The feeble, distant ticking, calling to mind the sounds of the fœtal heart, is most striking in large effusion into the pericardium, but it may be met with as a consequence of advanced degeneration and dilatation.

The interval between the first and second sounds may be shortened as well as lengthened, and there are at least three ways in which this shortening may conceivably be brought about. First, the ventricle may be supposed to contract with unusual vehemence so as to complete its systole more quickly. This occurs, no doubt, in excitement, in palpitation, &c., but the frequency of the heart's action is also greatly increased, the second pause is shortened as well as the first, so that there is no great departure from the ratio between the two intervals. But secondly, with or without increased energy in the contraction of the ventricle, there may be diminished resistance in the minute arteries and capillaries ; then the heart expecting, so to speak, a certain resistance, does not meet with it, and thus the systole is sudden and short. This I believe to be the explanation of some attacks of palpitation. I had under observation for a long time a medical man over the age of seventy, who had suffered much from gout. He had habitually high tension in his arteries ; the pulse of the wrist was long and firm, and between the beats the artery could be rolled under the fingers like a tendon, and could be followed far up the fore-arm. He became subject to violent attacks of palpitation, and it was found that during these the high arterial tension had disappeared, and the pulse was large, and soft, and short. I saw him several times when the attack was coming on, and it appeared to me that the first event was relaxation of the arteries, then the heart gave a

number of sudden, short, violent beats, with rather long unequal pauses between, as if it had contracted upon nothing; after which the rapid and violent action of palpitation set in.

In pyrexia there is diminished peripheral resistance as well as excited action of the heart, and this helps to make the systole short. Were the interval between the first and second sounds not thus shortened, the increased frequency of the pulse would tend to make the two pauses equal, but this is very rarely the case; on the contrary, in pyrexia the second sound often follows the first so closely that the systolic interval may be even shorter in comparison with the diastolic pause than in the normal state.

The third condition under which the interval between the first and second sound may be abnormally short is when the ventricle fails to complete its contraction and is unable to empty itself. This indicates extreme cardiac asthenia, or at least that the resistance in the arteries is greater than can be overcome by the heart, and is consequently of great clinical importance. It may be present in advanced fatty degeneration of the heart, or in acute dilatation of the heart, or in the late stage of chronic dilatation, succeeding the opposite condition of retarded second sound with tic-tac character of sounds; or when the nutrition of the walls of the heart is interfered with by aneurism of the root of the aorta, or by adherent pericardium. When the second sound follows the first immediately, the danger of fatal syncope is extreme.

In July, 1882, a man, aged about fifty, was brought to St. Mary's Hospital suffering from diarrhœa. He was thin, pale, and sallow, and very weak. It was supposed that the diarrhœa and imperfect nourishment accounted for his illness. The purging became less frequent and severe, and there was a slight improvement in the general condition of the patient. One day, however, when in the ward, I was called to him; he had just been to the closet and had been seized with faintness. He was conscious, but ghastly pale, the hands, feet, nose, and ears cold, the face and chest bedewed with a cold sweat, the pulse scarcely perceptible. On listening to the heart the sounds were distinct though weak, but the first was short and was followed instantly by the second. This was readily recognised by several students. He never rallied, but died within an hour in spite

of all that could be done by stimulants, &c. It was found on *post-mortem* examination that the pericardium was universally adherent, and that the muscular structure of the heart, especially of the right ventricle, was far advanced in degeneration. It is not pretended that the recognition of the imperfect systole contributed materially to the prognosis; there were other signs sufficiently indicative of immediate danger, but it enabled us to realise the exact condition of the heart.

In April of this year I was consulted by an Irish gentleman on his way home from the south of England where he had been sent on account of a troublesome cough. I found signs of aneurism in the arch of the aorta, and the diagnosis was confirmed by the development of a pulsating tumour which eroded the sternum and proved fatal in August. In this case there was evidence of the high arterial tension which had led to the giving way of the aorta in visible pulsation in both radial and ulnar arteries of both wrists, and these vessels were still full between the beats, although the beat was very short. The interval between the first and second sound was also greatly shortened, and the systole evidently did not complete itself, probably because the nutrition of the heart had suffered from interference with the coronary arteries.

In another case seen last month in which I diagnosed aneurism of the transverse part of the arch of the aorta, the second sound followed the first at about the normal interval. The patient had suffered from severe attacks of angina, and he died in one of these a week after I saw him. Unfortunately there was no examination after death.

It is perhaps in acute dilatation of the heart that the recognition of uncompleted systole is most important. In the winter of 1880-1 a gentleman aged about 55, stout and healthy-looking, but gouty, and having habitual high arterial tension, after sitting up for the greater part of the night had a long cold drive without breakfasting. Gouty inflammation came on in the knee, and while suffering from this he was seized with intense pain and oppression in the chest, faintness, and dyspnœa. Next day, when he got out of bed to pass water, he was so helpless and weak that he had to be lifted into bed, and almost died in the process. When I saw him on the fifth day after the

attack he had been treated by stimulants, and had rallied somewhat, but on feeling the pulse I found instead of the long and powerful beat, and the sustained high tension habitual to him, a weak short pulse, and although the artery was large and full between the beats, it was easily compressible, and the wave was arrested by very slight pressure. The heart, again, instead of a good impulse and defined apex-beat, gave only a weak diffused tap against the chest-wall, and the second sound followed the first almost immediately. It was obvious that the left ventricle never emptied itself, and that its contraction was inefficient and was arrested by the resistance in the arteries. By free mercurial aperients on the one hand and digitalis and ammonia on the other, he gradually recovered, and has since been in full and laborious work.

I am anxious that the points brought forward should not be looked upon as mere diagnostic refinements, difficult to make out, and useless when recognised. They are, on the contrary, very easy of recognition, and I can testify to their great interest as giving additional precision to our knowledge of the anatomical condition and functional competence or incompetence of the heart, and as affording important aid in directing therapeutic measures. They demand, of course, thought and care on the part of the observer in forming and applying conclusions based upon them, and perhaps the more readily signs are made out the greater the caution required in drawing inferences as to their significance, but the same is true with regard to murmurs, and indeed with regard to symptoms and physical signs generally.

I may perhaps be permitted to relate one more case seen on the day on which this communication was read at the Harveian Society. A gentleman, aged 52, from the West Indies, a large eater but very moderate drinker, who lived much in the saddle, consulted me on the recommendation of Mr. A. P. Boon, in December, 1880. He had fallen down unaccountably once or twice, and had exhibited strange mental symptoms, such as unfounded suspicions. He was better for the voyage, and looked robust physically, but complained of want of breath which caused him to sigh deeply from time to time. The condition of the circulation at once attracted attention ; the pulse was frequent and

tense but short, though the artery was full between the beats, and the temporal arteries were tortuous. On examining the heart it was found to be very large, the area of deep dulness being extensive, and the apex beating diffusely in the sixth or seventh space three inches below the nipple and to the left of the nipple line. The sounds were equidistant, the first being reduplicated near the apex, while the aortic second was loud with a ringing tone, and was audible along the right edge of the sternum from the fourth space upwards, and again with similar characters at the apex. The interpretation placed on these signs was that there was great resistance in the arterioles and capillaries, and that this had given rise to dilatation of the aorta and of the left ventricle, and further that the ventricle was no longer fully competent to cope with the obstruction. The cerebral symptoms were probably due to imperfect supply of blood to the brain. The patient went to Scotland, and was not seen again till June, 1881, when he was better in general health, but still had suspicions that he was watched, and complained of breathlessness, not so much after exertion, as after meals. The tension in the pulse was now more marked, and the impulse and apex-beat of the heart better and more defined. The symptoms were now quickly shaken off under treatment which was mainly eliminant, and he returned to the West Indies, but was soon compelled to leave, and in March, 1882, he again consulted me. There had been no recurrence of the delusion, but the sense of breathlessness was again troublesome, and once or twice "his legs had disappointed him" and he had fallen. The pulse and cardiac signs were much the same as before, the beat of the pulse being short though the artery was full in the intervals and tense, and the evidences of dilatation of the aorta and of the left ventricle very striking. New points noted were that reduplication of the first sound could be followed upwards for some distance along an oblique line much within the apex, apparently corresponding to the septum, and that a curious double beat could be seen, but not felt, below the left seventh costal cartilage in the epigastrium. In May and June the breathlessness was still complained of, and the patient was observed to take several deep and noisy breaths while under examination. On July 21st he had an attack of giddiness, in which he could

not see distinctly, and could not guide his limbs properly. It was followed by epistaxis, after which his pulse became soft for a time, and he had no breathlessness. In August, double vision suddenly came on, but lasted only a day or so. On October 6th he was better, had lower tension in the pulse, and very little sense of breathlessness, and he remained remarkably well till November 1st. He had become careless and had allowed the bowels to be constipated, and on this day was so much occupied in writing for the mail that he took little food in the middle of day, but in the evening he made up for this by an unusually heavy meal. Soon after eating this he was seized with sudden pain in the chest and faintness, and when he was seen by Dr. Herbert Snow the pulse was weak and flickering, and the action of the heart tumultuous, a faint diastolic murmur being audible. An emetic was given and then digitalis and ammonia. Under this he rallied, but there was well-marked Cheyne Stokes respiration. I saw him at 2.30 next day. He was still restless, anxious, and oppressed, the Cheyne Stokes breathing continued, with pauses of twenty seconds duration. The pulse was irregular both in force and frequency, and on examining the heart an extremely loud double aortic murmur was heard both at and near the apex, and in the aortic area. There could be little doubt that an aortic valve had been ruptured, and the patient died suddenly that same evening.

In this case the pressure within the arterial system was such that injury of some kind was inevitable; the aorta was already greatly dilated, and the left ventricle dilated and hypertrophied. At one time it seemed as if the heart were giving way, at another as if rupture of some cerebral vessel were imminent. The final and fatal event, however, was rupture of an aortic valve, which takes place very rarely.

ON THE ANTAGONISM BETWEEN VERATRIA AND POTASSIUM SALTS.

BY SYDNEY RINGER, M.D.,

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In some previous experiments published in the *Journal of Physiology*, Vol. III., No. 5, I found that circulating simple saline solution through the ventricle causes it after each contraction to dilate very slowly; in fact the trace is just like the trace of a ventricle poisoned by veratria. I showed also that a small dose of any potassium salt, 1 in 10,000 to 1 in 15,000 parts, will obviate this prolonged dilatation, making the ventricular dilatation normal.

I subsequently learnt that the saline solution supplied to me was made with pipe water instead of distilled water, and on testing the action of saline solution made with distilled water I did not obtain this dilatation. The prolongation of dilatation of the ventricle is therefore due to some of the constituents in the pipe water. The water used in making my saline solution was supplied by the New River Water Company, and contains the following inorganic constituents in a million parts: calcium, 38; magnesium, 4·5; sodium, 23·3; potassium, 7·1; combined carbonic acid, 78·2; sulphuric acid, 55·8; chlorine, 15; silicates, 7·1; free carbonic acid, 54·2.

I have made numerous experiments which clearly show that the minute trace of lime in this water is the cause of the prolonged ventricular dilatation.

The effect of a minute quantity of a potassium salt in removing or preventing lime salts prolonging the diastolic dilatation of the ventricle, naturally led to experiments to test whether

potash salts can prevent veratria prolonging the ventricular dilatation.

These experiments were made with a Roy's tonomer.

In each experiment I used 100 cc. of blood mixture, made by dissolving dried bullock's blood, so as to represent normal blood, diluted with four parts of saline made with New River water.

The potassium chloride solution contained 1 per cent. of the salt.

I find that a small dose of potassium chloride will completely obviate the effect of veratria and restore normal contractions.

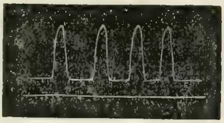
On June 12, after taking a normal trace, I added to the 100 cc. of circulating blood mixture 1 cc. of 0·2 per cent. solution of veratria, which produced well-marked veratria effects. About twenty minutes after the dose of veratria I added 2 cc. of potassium chloride solution, and in about half a minute this greatly lessened the veratria effects. Then from time to time I added more of the potassium chloride solution, and after the addition of 5 cc. the contractions became normal and only a little weaker than the contractions previous to the addition of veratria. On June 13 I neutralised the effect of 5 cc. of 0·2 per cent. veratria solution by adding to the veratrised circulating blood mixture 3 cc. of 1 per cent. solution of potassium chloride.

On the same day, after taking a normal trace, I added to the circulating blood mixture 2 cc. of 0·2 per cent. solution of veratria and produced a well-marked veratria effect. Five cubic centimetres of potassium chloride solution completely counteracted the veratria, and restored good normal contractions. About forty minutes later I added a second dose of 3 cc. of veratria solution and a second time produced marked veratria effects. I then added a second dose of 3·5 cc. of potassium chloride solution, and a second time counteracted the veratria and restored normal contractions equal to the contractions at the beginning of the experiment when the unpoisoned blood mixture passed through the ventricle.

On another occasion, after taking a normal trace, I added to the circulating blood 1 cc. of 0·2 per cent. veratria solution, with 3 cc. of solution of potassium chloride, and the contractions

continued unaltered. Then I added three successive doses of 1 cc. veratria (0·2 per cent.) solution, making 4 cc. in all. This produced a moderate veratria effect. Then I added 1 cc. of potassium chloride solution, and counteracted the veratria, but the contraction became weaker.

On June 14, after taking a normal trace, I added to the circulating blood mixture 3·2 cc. of 0·2 per cent. solution of veratria. This large dose of course produced well-marked veratria effects, which were entirely removed by 5 cc. of the potassium chloride solution.



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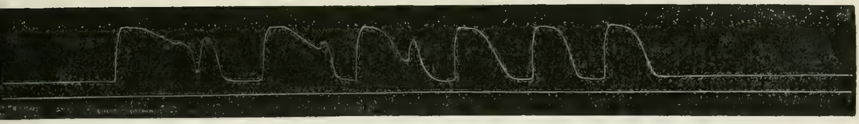

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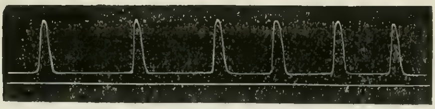

C

Description of Tracings.—A, trace with blood mixture; B, trace after the addition of 3·2 cc. of 0·2 per cent. solution of veratria; C, trace after the addition of successive doses of 1 per cent. potassium chloride solution, 5 cc. in all.

NOTES ON THE TREATMENT OF ULCERS.

BY JAMES WHITSON, M.D., F.F.P.S.G.,

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An ulcer may be defined as any breach of, or solution of continuity in the skin, other than a recent wound; and the treatment of these is interesting and instructive to the patient observer. In the carrying out of this, our great aim is to make the sore a healing one, and ulcers occasionally fail to progress satisfactorily from excess, as well as from defect of action, while those which are syphilitic are often tedious even though the patient is put under the influence of specific remedies. Varicose ulcers, due to a weakened or dilated state of the veins of the leg, are extremely common, and the callous ulcer with its high margins and depressed centre are frequent inmates of most large hospitals.

Healing begins at the edges, and is centripetal in its action. In this way the healthy sore is distinguished by its blue periphery and even surface. In its treatment little is required except a dressing of slightly damp lint, careful bandaging, and a studied position and rest of the limb. In carrying this out, however, there are one or two points which require attention, and which materially accelerate complete cicatrization. The lint should be accurately cut to the size of the sore. It should not overlap the sound skin round its margin, for by this means the tender granulations are kept in too moist a state for successful progress. The water-proof tissue which is put on above should extend slightly beyond the dressing in order to prevent evaporation, and the whole is retained in its place by means of

an ordinary roller of calico. Careful bandaging is essential so that the pressure may be applied equally all round, and no "reverses" should be put on over a sore, as they bear unduly on one part.

Where the ulcer is defective in its action we must take steps to increase it. If the edges are high we must depress them to their proper level, and there is no more effectual method of accomplishing this than by blistering. The Emplastrum Cantharidis, Liq. of Smith, is a most convenient vesicant, and it should be painted over the elevated margin by means of a camel's hair pencil. If necessary, it can be repeated in the course of a few days. At the same time the dressing should be dipped in a stimulating lotion, and of these we have several to choose from. One of the best is the potassio-tartrate of iron, which acts constitutionally as well as locally, and it may be used of several degrees of strength, as from ten to thirty grains to the ounce of water. Hay's wash, which consists of two grains of sulphate of zinc to a like quantity of water (along with lavender and rosemary), as well as a solution of hydrate of chloral (grs. x. to ℥i. water), are favourite applications with many surgeons. Black wash, the lotio nigra of the Pharmacopœia, is generally approved of in syphilitic cases, but care should be taken to shake the bottle in which it is kept before using it, as the calomel from its weight sinks to the bottom and if undisturbed the mixture when poured out will be quite clear. The lint should not be applied in too moist a state, as it is then incapable of absorbing the discharge, and the sore will consequently become bathed in it. It is best to renew the dressing twice daily, and when doing so, if it is adherent to the granulations never use force in its removal, but soften the adhesions by means of a few drops of water from a syringe.

The sprinkling of iodoform or calomel over specific sores, combined with a dry dressing, is seldom without good effect, and undermining of the edges is a frequent characteristic of this class of affections. When such is the case the burrowing sinuses should be slit up without delay in order to induce healing from the bottom.

The action of the exuberant sore must be repressed, and sulphate of copper or nitrate of silver will generally be found

efficient agents for bringing down its surfaces to the level of that of the surrounding skin.

In treating the callous ulcer a pretty vigorous line of procedure is necessary. These are generally situated on the leg, and are not always easy to cure. The edges are invariably high, with a correspondingly depressed centre, and their condition is in most cases aggravated by the patient going about too long, and by insufficient dressing and bandaging. The elevated margins must be freely blistered until the whole extent of the granulations is reduced to the same height, while a stimulating lotion ought to be applied to the sore, and great care exercised in the bandaging of the limb. In the treatment of most ulcers an occasional change in the dressing is beneficial, any one remedy losing its effect after a period of constant application. It need scarcely be added that strict attention to the state of the constitution is always of the utmost importance, and if the secretions become in any way deranged immediate steps must be taken to regulate them.

I have frequently found marked benefit resulting to an ulcer from strapping. The strip of plaster should extend completely round the leg, and it may with advantage be put on both above and below the affected spot, care being always taken at the same time to see that the circulation in the limb is in no way compressed or impeded. Another method which I have often seen productive of good is the application of a splint, and it should possess a foot-piece in order to prevent any movement of the muscles of the part. In very slowly healing ulcers the incising of the skin round the sore, at a short distance from its edge, is a method which generally assists cicatrization ; and in some chronic cases which resisted nearly all treatment, I have on more than one occasion found this procedure to result in improvement. The reasons for this are not far to seek, the chief being the relief of tension and the freeing of the blood-vessels from peripheral pressure, while the approximation of sound skin must tend to shorten the period of reparative action. A wedge pillow may be placed under the limb, as it favours the return of blood to the heart, and ulcers in a congested state are disadvantageously circumstanced for satisfactory progress. It frequently happens that patients come into hospital

in this latter condition owing to a long continued debauch, and in these cases the best local application for the first few days consists of a lead and opium lotion, along with absolute rest and elevation of the limb. The bowels at the same time ought to be freely opened by a mercurial purge, repeated if necessary before proceeding with the ordinary curative measures. Recently I have now and then treated ulcers antiseptically, and the results in all instances were good. The cases were by no means favourable ones, so that the test might be considered a satisfactory and encouraging one. The absence of all irritation, the porous nature of the gauze, and the infrequent change of dressing, are all important factors in the restoration of healthy action, and doubtless contributed greatly to the desired end.

On the Continent the application of a specially prepared sand[1] to granulating sores has been tried for some time with success, and, as it possesses the advantage of continuously absorbing the discharge, seldom requires removal, while cicatrization can thus proceed without interruption. When the cure of ulcers is complete it is well to give the affected limb steady support by means of a bandage, and for this purpose the india-rubber ones of Martin are admirably adapted. They are preferable to calico, and while accommodating themselves readily to any muscular movement, are at the same time easy of application and agreeable to the wearer. They should always be put on before the patient gets out of bed in the morning. In this way the parts are not likely to become congested, the pressure of blood on the veins is considerably lightened, and the liability to the formation of fresh sores materially diminished.

[1] The sand is prepared by first of all heating it to a temperature capable of destroying all its organic particles. It is then soaked in a solution of bichloride of mercury and water, the proportions used being one part of the former to one thousand of the latter. After this the mixture is placed in bottles, and can be used when required.

A CASE OF PERNICIOUS ANÆMIA, WITH REMARKS AND BRIEF NOTES OF FORTY-FIVE CASES OF THE SAME DISEASE ARRANGED IN GROUPS.

BY J. M. HOBSON, M.D. EDIN.

TILL I went into the literature of pernicious anæmia, only a short time since, I had the idea that this disease was not very interesting. I thought that the knowledge we had of it was mostly negative; that there was no cause, no cure, and no pathology. Had I known what I do now, my attitude towards the case which I shall presently relate would have been different, and, perhaps, a valuable life would have been saved.

I had seen a case before, when house-physician at Guy's Hospital, and the idea that this might be of the same nature did occur to me, yet I thought that the balance of evidence was in favour of a simple anæmia, and so did a practitioner of well-merited repute who saw the patient with me. We therefore gave a favourable prognosis. The progress of the case undeceived me.

A Case of Pernicious Anæmia developing after Menorrhagia.—Miss M., aged twenty, in comfortable circumstances, had always been a delicate girl. I was told that her mother had been ill-used by her father, who had half starved her while pregnant with this child. I attended her about twelve months before, when she was suffering with backache—uterine or ovarian—and had some degree of anæmia. After taking Bromide of Potassium and Syr. Ferri Phosph. Co. for several weeks, she considerably improved in both respects. Unfortunately the improvement did not continue. At the end of last May she came to see me, walking two miles to my house and the same distance home again, I believe. She was then very anæmic,

the last menstruation had been profuse, she had had a cough, which was now better, and with which she had expectorated blood-stained mucus. The expectoration of blood-stained mucus continued, though it was rather hawked up now than expectorated. Physical examination of the chest revealed nothing abnormal. Three days later I was sent for to see Miss M., and found her decidedly worse, unable to make the least exertion without throbbing of the head, extremely pallid, and bringing blood from nose and mucous membrane of mouth in small quantities. I ordered her Liquid extract of Ergot and Liquor Ferri Perchlor:. On June 5th—five days later—I found the bleeding had not been appreciably influenced by the ergot by the mouth, and menorrhagia had returned, though the period was not due. I therefore gave grain $\frac{1}{3}$ of *Ergotine* (Savory and Moore's discs) subcutaneously, and more than doubled the quantity of iron. 6th, the ergotine had given rise to some local irritation, so I ordered $\frac{2}{3}$-grain doses to be given by the mouth on an empty stomach, and the iron mixture to be continued. 8th, the bleeding had received a check—Ergotine $\frac{1}{3}$ grain twice daily. 10th, the bleeding had now stopped, so ergotine was discontinued and the dose of iron increased. By this time there had been improvement in the general condition, and I was hopeful, even confident, of recovery. Rather profuse leucorrhœa remained. 13th, patient passed two large clots with much pain and exhaustion. 14th, a neighbouring practitioner, above mentioned, saw the patient with me and advised a return to the ergotine, but omission of the iron. The menorrhagia had returned. Patient at once began on half a grain of ergotine three times a day. The head was now very bad when she sat up. 16th, no check to the hæmorrhage.

℞. Extracti Ergotæ liquid. ʒvi.
Potassii Bromid. ʒij.
Aq. ad. ℥vi. ℥ss. 4tis. horis.

On the evening of this day I was sent for, as Miss M. was thought to be dying. I found her in a hardly conscious state, tossing about frequently, with a gasping or sighing respiration and a pulse of 140 or more. I at once injected ten minims of brandy subcutaneously, again in half an hour, and again after an

hour more. She began to revive after the first injection, and when I left, five hours later, she had considerably improved. The hæmorrhage had continued unchecked till, after coming round a bit, she had one grain of ergotine injected into the deltoid muscle. She also had some of the mixture. Before I left the hæmorrhage was considerably checked.

On the 17th she kept hardly anything down.

The vomiting after this was kept in check by ice and lime-water, yet, though she improved somewhat for a few days, she never regained her position of the 14th. The hæmorrhage did not again trouble us—the ergotine injections were repeated once or twice—though the leucorrhœa persisted.

On my morning visit on the 23rd, I found Miss M. breathing at a tremendous pace, and evidently much distressed by breathlessness. The pulse was very small and frequent—for the last two days it had been getting more frequent and I had added ♏10 of Tinct. Digitalis to each dose of mixture, the day before, hoping to quiet the pulse. Ten minims of brandy were injected twice, but without any effect. Spirits of ether, ♏30, were then given, but without any benefit. A few hours later the breathing was still distressed and the surface was getting cold. Consciousness was not in abeyance, but she was too low to give expression to it or to take notice. She died at 8 P.M. The anæmic look had become very great, but it was not of the pure white one as seen in other cases—it had a certain yellowish or greenish tinge.

Looking back on this case we might comment in the following manner. A young woman with, probably, always too few red globules in her blood, menstruates too freely once, or, perhaps, oftener, and thus farther reduces her stock of blood-cells. This reacts on her blood-forming organs, amongst the rest, sets up a *circulus viciosus*, as Quincke puts it, and, henceforth, the supply gets more and more below the requirements of the system.

Dr. Coupland in his Galstonian lectures last year pointed out that the terms "Idiopathic anæmia" and "Pernicious anæmia" were not equivalent, and proposed the following classification:

$$\text{Anæmia}\begin{cases}\text{Symptomatic}\begin{cases}\text{Simple}\\ \text{Pernicious}\end{cases}\\ \text{Idiopathic}\begin{cases}\text{Simple}\\ \text{Pernicious}\end{cases}\end{cases}$$

In a considerable number a sufficient cause existed for the anæmic state, so that their severity warranted the use of the term *pernicious*, while the name idiopathic was inapplicable.

I have collected from the journals, and from a paper on "Idiopathic Anæmia," by Dr. F. Taylor, in the *Guy's Hospital Reports* for 1878, forty-five cases of pernicious anæmia. In the great majority the antecedent conditions were closely connected with the subsequent development of pernicious symptoms, while those in whom the symptoms arose, apparently without any antecedent cause, were only twelve. Even of these latter one cannot certainly say of some that there was no antecedent ill-health or evil environment which led up to and culminated in pernicious anæmia; as in a bricklayer who, for three months before he noticed the pallor of his face, was weaker and vomited (case 10); or a potman who had been subject to rheumatic pains since childhood (case 11). But when we come to consider the other larger class, we find the causal relation between the antecedents and the severe anæmia was much more obvious. For instance, three cases followed confinement, and one abortion. Quincke[1] observed that a certain number were connected with pregnancy, and that in these abortion generally took place in the seventh or eighth month, and then death quickly followed, without there being necessarily any great loss of blood. Gusserow's six cases, published in 1871, occurred in pregnant women. Still more obvious is the connection between hæmorrhage and this form of anæmia as seen in two of my cases where there was profuse menstruation. Referring to this point Dr. Coupland says: "A profuse hæmorrhage, which at the time may threaten life, leaves behind it an anæmia which never disappears, and may become pernicious—*i.e.*, may deepen in intensity in spite of treatment." In two of my cases there was kidney disease, and in connection with one of them there was, naturally enough, some difference of opinion at the Dublin Pathological Society, where it was brought forward by Dr. W. G. Smith, some thinking it was essentially Bright's disease, others that it was truly to be called pernicious anæmia. In three cases depressing circumstances only could be assigned as causes; one, probably poor, had tramped through snow; a second had been

[1] Translation in *Medical Times*, 1876, vol. ii. pp. 375 and 428.

"out of place, ill, and in poverty"; a third had been ill-used by her husband, and had suffered much grief. One case had been a chemical worker, caught cold, and then the symptoms of pernicious anæmia set in. Some of the cases, twelve in number, had been subject to prolonged ill-health in some form or other which had culminated in these pronounced anæmic symptoms. Quincke [1] says that the majority of his patients were in wretched circumstances, being barely sustained by bad and insufficient food. There had been other depressing agencies at work, not unfrequently, such as typhoid fever and protracted catarrh of the stomach and intestines. Dr. Coupland, continuing the remarks which I have just quoted, goes on to say: "More commonly this exciting cause is added to a pre-existing and long-standing anæmia, which then takes on a fatal form, or the determining event may occur in some other exhausting discharge. Leaving for the present the consideration of these prodromal influences, I may simply state now, that in many cases the onset is gradual and insidious, having perhaps in some cases a foundation in conditions of blood-impoverishment, but in others arising quite apart from such conditions." In another place Dr. Coupland, analysing the 110 cases he has collected, found that, in forty, no cause had been attributed. In eleven, bad living and food, such as potatoes and coffee, associated often with pregnancy and lactation, or gastro-intestinal disorder, had to do with the anæmia. In twenty, pregnancy, especially in women who had had their families rapidly, was assigned as the starting point. All this etiological observation, which has been accumulating of late years, goes to altering our standpoint with regard to Addison's *Idiopathic anæmia*, occurring in "cases in which there had been no previous loss of blood, no exhausting diarrhœa, no chlorosis, no purpura, no renal, splenic, miasmatic, glandular, strumous, or malignant disease.[2] Now under *Pernicious anæmia* one finds included cases in which one or another of nearly all that list of conditions were found. The reason for this is that the peculiar anæmia and internal fatty changes of Addison's classical description, with subsequently described blood-cell changes and occasional bone changes, are common to a variety of cases where there have been depressing influences, such as the above, at

[1] *Loc. cit.*

[2] Addison's orig. Paper "On Disease of Supra-renal Capsules," 1855.

work, or where no such influences have appeared. We know now that a considerable amount of adipose tissue does not always exist in the subjects of the disease, as Addison supposed. Neither is it so invariably fatal as he thought it. Addison's "Idiopathic anæmia" was a mysterious and hopeless disease; now it appears less as an undividual malady than as the culminating point of many diseases. Addison used the word *idiopathic* under protest, and I think, if he had been able to refer to 110 recorded cases, would gladly have given it up. The term *pernicious* is not entirely a desirable one, and is, perhaps, only transitional, but it does not in itself imply a fatal termination, only a *progress towards* a fatal termination. Cases which have, to all appearance, been as bad as some which have died, have completely recovered. If we adopt, then, the term *pernicious* in preference to *idiopathic,* we do not commit ourselves to any theory, but are merely expressing, till we can understand the pathology and, I may add, the physiology, of the blood better, a severe condition of bloodlessness—aglobulie of Hayem.

I have already thrown out a suggestion as to the *modus operandi* of a depressing agent in the case which I have quoted at length at the beginning of this paper. Dr. James Andrew [1] thinks that there is some personal factor which allows the anæmic process to become cumulative. Might this necessary link be a certain *weakness,* hereditary or acquired, in the organs responsible for the production of fresh blood-cells, whereby the resources within themselves are unduly limited? So long as these organs have a sufficient blood-supply they can keep up with the demand of the economy, but directly the supply, from whatever cause, fails, their power of cell-production fails also; their *output* falls off and the blood is proportionately impoverished. Thus the "circulus viciosus" is established. I admit that this "weakness" is hypothetical, yet the notion obtains elsewhere in pathology. The facts of leukæmia support the idea of "weak" *blood-cell nurseries.* Some of my cases had leukæmia.

The necrosis of blood-cells, as evidenced by the abundant débris and the excess of iron in the liver and other organs, is sufficiently accounted for by the ill-developed and perishable condition of the red corpuscles without looking for some *cytoclast* of a ferment, as Dr. Lauder Brunton suggests.[2]

[1] *Medical Times,* 1877, vol. i. p. 471.

[2] *Practitioner,* vol. xxiv. p. 107.

Appendix, containing Notes of Forty-five Cases arranged in Groups according to their Antecedent Conditions.

Group I., in which the antecedents had apparently little or nothing to do with the causation. This comprises twelve cases, which I have arranged in the order of their apparent spontaneity, putting those first which seem most "idiopathic."

Case 1. Male; æt. 42. Antecedents—Labourer. No illness till three months before admission, then ordinary symptoms developed. Result—Died. P. M.—Much adipose tissue, heart enlarged, with "tabby-cat" degenerations. Remarks—Appetite good at first; systolic murmurs heard all over chest and in back; but little pyrexia.

Case 2. Male; æt. 10. Antecedents—In good health till six months before death, no apparent cause for illness. Result—Died. P. M.—Liver mottled with parenchymatous hepatitis and hæmorrhages into fattily degenerated patches, *marrow of ribs and clavicles of a red colour.* Remarks—Exudation and hæmorrhages into both retinæ, epistaxis and bleeding from gums.

Case 3. Female; æt. 42. Antecedents—Servant. Five months before, began to feel weak, to grow pale, and to lose appetite and flesh. Result—Recovered under arsenical treatment. Remarks—Appetite good and no marked indigestion while under observation; amenorrhœa for some weeks before admission.

Case 4. Male; æt. 43. Antecedents—A turner by trade, had been ill a year before with pallor, weakness, loss of appetite, and sickly feeling in epigastrium; onset gradual. Result—Died in delirium. P. M.—Not recorded. Remarks—Hæmorrhages in retina appeared two months before death, improvement for a time under arsenic and hospital treatment.

Case 5. Male; æt. 31. Antecedents—Became gradually weak without apparent cause, then lost blood by stool. Result—Died. P. M.—Extreme fatty degeneration of all organs.

Case 6. Male; æt. 22. Antecedents—Said that he had always enjoyed very good health, but that a month ago he gradually became so weak that he could hardly walk, and that he became breathless; epistaxis a day or two before admission. Result—

Died. P. M.—Heart small, apparently no fatty change in any organs. Remarks—Appetite very good, severe epistaxis towards close.

Case 7. Male; æt. 52. Antecedents—For some months failing appetite, weakness, cough, lumbar pain, and occasional diarrhœa. Result—Died. P. M.—Heart fatty, mucous membrane of stomach, especially in pyloric region, thickened. Remarks—No excess of white corpuscles.

Case 8. Male; æt. 48. Antecedents—Shoemaker, under treatment nine months or a year before with anæmia, vomiting, and severe gastric pain, simulating cancer of stomach. Result—Speedily improved when put upon iron, arsenic, and nux vomica, ultimately quite well. Remarks—Temperature never above normal.

Case 9. Female; æt. 29. Antecedents—Hard work as domestic servant, no other cause apparent, seems only to have lasted eight weeks. Result—Died. P. M.—Sub-pericardial ecchymoses, walls of heart soft and flabby, no mention of fatty degeneration, mucous membrane in pyloric region thicker than elsewhere, white and perfectly smooth.

Case 10. Male; æt. 47. Antecedents—Bricklayer, three months before *whiteness* noticed, he felt more tired than usual and had pretty frequent vomiting. Result--Not known. Remarks—Pain and tenderness in bones of legs.

Case 11. Male; æt. 43. Antecedents—A potman, had been subject to rheumatic pains in all parts of his body since rheumatic fever in childhood. Result—Died. P. M.—No brain lesion, extreme fatty degeneration of muscular fibres of heart, some old pericardial adhesions, a few sub-pleural ecchymoses, capsules healthy. Remarks—Two transient attacks of aphasia and left hemiparesis, retinal hæmorrhages.

Case 12. Male; æt. about 65. Antecedents—Two or three years before his death he married for the second time a young wife by whom he had one or two children, no other cause assigned. Result—Died. P. M.—Not recorded. Remarks—Duration six months.

Group II., in which the antecedents seemed to have a closer connection with causation than the above, though not so close as in the next group. This comprises seven cases.

Case 13. Male; æt. 21. Antecedents—From childhood had always been rather pale and unable to take vigorous exercise, for some years had been liable to occasional slight epistaxis. Result—Died. P. M.—Usual appearances found in pernicious anæmia, ecchymoses under endo- and peri- cardium. Remarks—Transfusion at last stage with no good result, no excess of white corpuscles.

Case 14. Female; æt. not stated. Antecedents—Cook and housekeeper, unmarried, had been ailing for two years, losing colour *and flesh,* and suffering from shortness of breath, especially for the last six months, relapse after remaining fairly well for twelve months. Result—Died. P. M.—Not reported. Remarks—Appetite and digestion good, corpuscles eleven per cent. of normal, improved greatly under Ferri arsenias, first time, when corpuscles rose to 41·6 per cent., no retinal hæmorrhages.

Case 15. Female; æt. 46. Antecedents—Failing health with anæmia and weakness for three years. Result—Died. P. M.—Heart and liver fatty.

Case 16. Male; æt. 47. Antecedents—Engine-driver, skin began to get yellow about eighteen months before, worse and better since. Result—Died. P. M.—Heart said to be healthy on internal aspect.

Case 17. Female; æt. 51. Antecedents—Gradual failure of health for about one year, culminating in extreme anæmia. Result—Died. P. M.—Heart and liver fatty. Remarks—No excess of white corpuscles.

Case 18. Male; æt. 52. Antecedents—Slowly increasing debility with rheumatic pains. Result—Died. P. M.—Heart, liver and kidneys, fatty.

Case 19. Female; æt. 50. Antecedents—Inmate of lunatic ward for fifteen years, during last few months gradually fell into a state of great debility. Result—Died. P. M.—Heart, liver and kidneys, fatty.

Group III., comprising twenty-six cases, in all of which some antecedent conditions clearly caused or led up to the state of dangerous anæmia. I have collected these cases into smaller groups according to the conditions which preceded the anæmia.

(*a*) Some condition of prolonged ill-health. Twelve cases.

Case 20. Female; æt. 29. Antecedents—Married thirteen years, no pregnancy that she knew of, but some severe floodings. "Rheumatic fever" at thirteen. Severe fright twelve months ago. Three months ago seemed to have had urticaria, also sickness and jaundice. Not well since. Catamenia pale for the last three months. Result—Died. P. M.—Heart fatty, $11\frac{1}{2}$ oz. No mention of valvular disease. Liver fatty in *hepatic* areæ. Spleen 23 oz. No excess of white corpuscles, *p. m.* Remarks—Urine slightly albuminous. Pains in tibiæ, worse at night, spleen greatly enlarged on admission. Said to have had a great excess of white corpuscles at one time, but this went down. Hæmorrhage and diarrhœa at close.

Case 21. Male; æt. 27. Antecedents—Four years before had dyspeptic symptoms for nine months, and has since then not had such good health. During the third year was laid up with lassitude, pallor, emaciation, diarrhœa and abdominal pain, then improved for a time. Result—Recovered in a very marked manner under arsenical treatment. Remarks—Liver enlarged and painful. Temperature irregular, sometimes 104° to 106°. No excess of white corpuscles. No retinal hæmorrhages.

Case 22. Female; æt. 54. Antecedents—Mental shock and hæmorrhage from uterus led to ill-health for last five years. Result—Died, with diarrhœa. P. M.—None made. Remarks—Arsenic and iron together failed.

Case 23. Male; æt. 16. Antecedents—Not well for three years. Often had epistaxis. Result—Died. P. M.—Heart, liver, and kidneys fatty. Healing ulcers and cicatrices of others in large intestines. Remarks—No excess of white corpuscles.

Case 24. Female; æt. 52. Antecedents—Had been ill-used by husband and had suffered much grief. Result—Died. P. M.—Heart and liver fatty. Blood watery. Organs anæmic. Muscles dark. Liver and spleen had peritoneal adhesions. Remarks—Obscurely ill three months. Tawny, obscure complexion.

Case 25. Male; æt. 68. Antecedents—An attack of diarrhœa and vomiting eighteen months before. Began to fail one or two months later. Five months before admission had a "fit" with unconsciousness, and three similar since. Result—Died. P. M.—Heart fatty.

Case 26. Male; æt. 47. Antecedents—For last seven years dyspnœa; getting paler and weaker. Result—Died. P. M.—Heart and liver fatty. Remarks—Cutaneous ecchymoses. No increase of white corpuscles.

Case 27. Male; æt. 53. Antecedents—Ten years before had an attack like present—very weak, fainting fits, yellowish colour, very ill and delirious. Never been the same man since. Had occasional faintings with breathlessness and giddiness. Became rapidly worse during last three months. Result—Died. P. M.—A patch of pneumonic consolidation in upper angle of left lower lobe with a cavity, the size of a marble, in its centre filled with pus and of old standing. Heart fatty—seen under microscope, but not evident to naked eye. Some degeneration in kidneys, mostly in and around Malpighian bodies. Remarks—Troublesome diarrhœa, some pyrexia.

Case 28. Female; æt. 27. Antecedents—Always rather delicate and more or less menorrhagia and leucorrhœa since fourteen. During last year before admission had attacks of vomiting, diarrhœa, giddiness, and palpitation with intermissions. Result—Died. P. M.—Ecchymoses under dura mater and pleura. Not stated as to fatty degeneration. Remarks—Not stated when pallor first became marked.

Case 29. Male; æt. 7. Antecedents—Had never been a strong child. Scarlet fever four years before. About six months before admission grew very weak. Result—Died. P. M.—Advanced fatty degeneration of heart. Ecchymoses beneath pleura and into substance of lungs. Fatty degeneration in *hepatic* areæ. Remarks—Retinal hæmorrhages and optic neuritis.

Case 30. Female; æt. 41. Antecedents—Palpitation, shortness of breath, pain in stomach and constipation for two years, more or less. Pallor only after all this. Result—Not known. Remarks—Pain and tenderness in bones of legs.

Case 31. Female; æt. 40. Antecedents—Delicate, spare; needlewoman. For many years occasional vomiting. For eight years vomiting had been almost daily. Menstruation ceased at thirty-four.

(*b*) Organic disease. Three cases.

Case 32. Male; æt. 49. Antecedents—Acute rheumatism

fourteen years before. In hospital a few months before with pleuritic effusion. Result—Died. P. M.—Old phthisical cicatrices in apices of lungs. Heart fatty, 17 oz. Old disease of aortic valves as well as recent vegetations. Spleen, 23 oz. Kidneys, 19 oz. Cortex whitish yellow and mottled. Remarks —Trace of albumen and fragments of granular casts in urine. No dropsy. General anæmia.

Case 33. Male; æt. 55. Antecedents—Illness dated from about nine months back. Result—Died. P. M.—Heart, 15 oz. Left ventricle hypertrophied. No mention of fatty change in heart. Kidneys cirrhotic to a certain extent. Medulla of femur, a soft reddish pulp with abundance of lymphoid cells. Remarks—Retinal hæmorrhages.

Case 34. Male; æt. 51. Antecedents—A tailor. Said he had good health till ten months before, then was taken ill and had to remain at home for several months. Got better for a time, then worse again. Result—Died. P. M.—*Chronic fibrous disease of left lung.* Heart fatty, blood in its cavities like claret-and-water. Numerous effusions of blood in subarachnoid space, purpuric spots in pericardium.

(*c.*) Drain from ulcers.

Case 35. Female; æt. 32. Antecedents—Cook twelve years. Ulcers of leg three years before, which recurred two years later and do not seem to have healed since. Menstruation ceased six months ago—lost ground since and lost colour also. Result—Died. P.M.—Cutaneous ecchymoses. Heart fatty.

(*d.*) Hæmorrhage.

Case 36. Female; æt. 21. Antecedents—A nurse. For some years had very little colour. Excessive menstruation latterly—had been flowing for ten days before admission. Result—Died, exhausted from hæmorrhage. P. M.—Extensive subpericardial ecchymoses, purpuric spots over kidneys. Heart extremely fatty. Remarks—Hæmorrhage became profuse and was not stopped before death. In five days the corpuscles fell from 49 to 20 per cent.; petechial eruption appeared about trunk and limbs before death.

Case 37. Female; æt. 20. Antecedents—Always delicate. Profuse menstruation latterly. Result—Died. P. M.—None made. Remarks—Recorded in full in beginning of this paper.

(*e.*) Parturition or abortion. Four cases.

Case 38. Female; æt. 34. Antecedents—Confined three months before, good time and well till then. Result—Died. P. M.—Oil-cells or oily molecules on walls of meningeal and pulmonary veins. Large lymphatic glands in posterior mediastinum, pressing on right vagus, thoracic duct, and splanchnics; some degeneration of right splanchnic. "Large portion of capsules appeared like so much putty." Heart large, cavities markedly pale, but healthy, apparently. Globules of fat in portal vein. Remarks—White corpuscles as much as one to ten of red, but this proportion varied. Glands in neck and axilla much enlarged. Spleen enlarged. Maximum temp. 104°·8. Severe epistaxis. Marked anæmia, a lemony hue during life, "with a patch over the left brow of yellow pigmentation about the size of the palm of the hand." ? Any complication with Hodgkin's disease.

Case 39. Female; æt. 31. Antecedents—Recently confined. Result—Died. P. M.—Fatty change in heart, liver, and kidneys. Remarks—Diarrhœa with blood; vomiting with loss of blood. Epistaxis.

Case 40. Female; æt. 28. Antecedents—Delicate health with much domestic trouble. One or two previous miscarriages with hæmorrhage. Aborted at three months with violent flooding. Result—Sank about one month after abortion. P. M. —Heart, liver and kidneys, fatty.

Case 41. Female; middle aged. Antecedents—"Always enjoyed good health until about nine months ago, when she had a very bad confinement, since which time she has never felt well, and has suffered greatly from epistaxis which comes on periodically every three or four weeks and lasts for several days."[1] Result—Recovery under arsenic. Remarks—Profound anæmia and prostration. Hyperpyrexia, for which quinine was taken. Liquor arsenicalis ♏ iij. ter die.

(*f.*) Directly exciting causes other than those above-mentioned. Four cases.

Case 42. Male; æt. 38. Antecedents—Chemical worker, home in a smoky atmosphere. Became exposed to wet and cold, then a rigor and symptoms followed. Result—

[1] For the Notes of this case I am indebted to Dr. Withers Moore.

Recovered under arsenical treatment. Remarks—No improvement till arsenic was begun. Dose, ♏ ij. increased to ♏ xvj. Retinal hæmorrhages. Size of heart increased, several bruits. Cardiac murmurs disappeared and blood became natural before discharge from hospital.

Case 43. Male; æt. not stated. Antecedents—Probably poor, tramped through snow. Result—Recovered under rest, improved diet, hydrochloric acid, quinine and iron. Remarks —Pallor developed while in hospital. Hæmorrhages and exudation into retina. Pyrexia. Increase of white corpuscles; five days after admission they were 40,000 per cmm., three or four days later 17,000, then, with some oscillation, 7,000. While in hospital an abdominal tumour rapidly formed and rapidly disappeared. Slight albuminuria. This case quoted from Lachman by Dr. S. Mackenzie, *Lancet*, 1878, vol. ii. ? Lymphatic leukæmia.

Case 44. Male; æt. 44. Antecedents—Out of place, ill, and in poverty. Result—Died. P. M.—Heart fatty. Remarks—No excess of white corpuscles.

Case 45. Female; æt. 15. Antecedents—Never been strong, yet not laid up. Seven months before, became weak and unable to work; had a bad attack of eczema. When eczema was cured was left very weak with sickness. Result—Died. P. M.—Heart fatty. Spleen, stomach, and supra-renal capsules healthy. Liver and kidneys pale. Mesenteric glands very large, Peyer's patches marked. Slight pigmentary dotting of spinal cord and medulla. Remarks—Body wasted. A bronzed appearance of skin after death, especially about axillæ and genitals.

THE TREATMENT OF DELIRIUM TREMENS.

BY F. P. ATKINSON, M.D.

In spite of all that has been written of late upon the treatment of delirium tremens and the necessity of supplying the system with nourishment, sufficient attention is not as a rule given to this point. At the same time, it is satisfactory to find the old idea that it requires to be treated on homœopathic principles —*i.e.* that it is necessary to keep up the vital powers by the administration (even if it be in diminished quantities) of that which has been the *fons et origo mali*, wellnigh exploded, and moreover to have it constantly on record that opium, digitalis, capsicum, &c., are powerless by themselves to effect a cure, inasmuch as it makes the thinking minds consider whether the remedies which are usually employed are really suited to the existing pathological condition, and if they fail in their purpose why it is they do not accomplish their end. In the first stage of alcoholism the appetite being fairly good, and the blood corpuscles in fairly normal proportions, chloral is quite sufficient to calm the excited circulation and produce sleep; but in the second stage, when the appetite is bad, the blood corpuscles deficient in quantity and shrivelled, and the brain anæmic and starved, it is useless to expect relief from sedatives unless the brain is at the same time supplied with the nourishment it requires. Death, no doubt, in delirium tremens arises from want of sleep, but then it must be remembered that the want of sleep is caused by want of nourishment. The most important part of the treatment then is to improve the quality of the blood as quickly as possible by throwing into the system frequent supplies of light, nourishing, and easily digestible

food, and the best way of accomplishing this end is by cutting off all stimulants and ordering half a tin of Brand's liquid essence of beef, and half a pint of milk to be taken alternately every two hours. As regards medicine, twenty-five grains of chloral with thirty minims of compound tincture of cardamoms in an ounce of water taken every four hours, after the beef tea, will be found most useful. Very little effect, though, is produced by the first dose of the chloral, inasmuch as the brain is without the nourishment it requires, but after the second dose the food begins to tell—some sleep, generally speaking, results, and this goes on increasing in proportion as the support is maintained. If nourishment is withheld, sleep disappears, and the old delirium returns.

By this treatment the patient is generally free from all delusions in about thirty-six hours, but good strong liquid food must still be taken for some days, though not quite so frequently. When there have been from ten to twelve hours more or less continuous sleep, then it is advisable to give up the chloral, and give thirty minims of the compound tincture of gentian with five minims of the tincture of nux vomica three times a day for about three days. This restores the tone of the nervous system and stomach, and creates an appetite. A little tincture of euonymin may next be substituted for the nux vomica, and some Carlsbad salt may be given in the morning when required. By this treatment the duration of the delirium and the after effects of the alcohol are very much lessened.

Reviews.

In-knee (Genu valgum). By W. J. LITTLE, M.D. London: Longmans, Green & Co. 1882.

THIS is an interesting book on the in-knee distortion, and its relation to other diseases. The fact that genu valgum is not of necessity connected with rickets is perhaps repeated a little too often by the author. Most of us have seen the in-knee distortion manifest itself at the age of fifteen or later, and in tall children who could not be described as rickety.

The remarks on growth at different periods of life are well worthy of study. The disappearance of the distortion when the knee is bent is explained by the statement—"that however much the condyles of the femur in knock-knee may differ in length perpendicularly, they remain of the same length antero-posteriorly; therefore, when the patient bends the knee the posterior surfaces of the condyles present in a horizontal plane towards the articular facets of the tibia; but when the patient assumes the erect posture, or extends the knee, the lower ends of both condyles impinge upon the tibia."

A feeling of disappointment will come over the reader in perusing the last third of the volume, on Treatment. With children simple apparatus and suitable movements will often suffice, but with adolescents the instrument-maker and his costly apparatus appear to be necessary. Dr. Little is to be thanked for so clearly pointing out the kind of instrument required, and the importance of securing a straight knee by its use. That cases have been cured by treatment in the bent position, and by the tailor's occupation—the patient sitting *à la turque*—allow us to hope that some method of treatment applicable to the poor will succeed in curing in-knee without costly apparatus. Few cases require osteotomy, and Dr. Little has wisely advocated gentleness and patience.

Regional Surgery, including Surgical Diagnosis. Part I. (Head and Neck). By F. R. SOUTHAM, M.A., M.B., F.R.C.S., Manchester. London: Churchill. 1882.

REGIONAL SURGERY is, as the author in his preface tells us, "intended for the more advanced student, and requires some

previous acquaintance with the principles of surgery; it is to supplement and not in any way to supplant the various text-books on this subject, in conjunction with which it is intended to be read." By all who read it, whether junior surgeons or general practitioners, it will be found a valuable and useful guide.

The plan upon which it is worked out is an admirable one. Under a number of sections are grouped together the principal surgical affections peculiar to each region of the body, and the care with which this is done is most noticeable. Turn, for instance, to Chapter I., on tumours of the scalp: nodes are considered under three headings—syphilitic, rheumatic, and strumous—but the traumatic are not forgotten, and that this form, simple though it be, is not always recognised one knows by experience. Upon more than one occasion it has happened that a node, the result, for instance, of pressure from a badly-fitting hat, has been mistaken for a syphilitic lesion, and active specific treatment adopted. Then the rare condition which Mr. Clement Lucas drew attention to in the Guy's Hospital Reports for 1876-1881, of collections of cerebro-spinal fluid under the scalp, the result of simple fracture of the vault of the skull, has not escaped notice. Osteitis deformans in connexion with hypertrophy of the cranium has its leading symptoms shortly and pithily described. Under affections of the face, attention is called to the prominent characteristics of myxœdema. At intervals in the book are to be found tables in which, with a view to differential diagnosis, the marked symptoms of various diseases are arranged side by side. These are well done; and we would refer with especial satisfaction, as a fair type of the whole, to that on page 34, showing the leading characteristics of lupoid ulcer, rodent ulcer, and epithelioma, as affecting the face.

One suggestion we would make with reference to these tables, namely, that an additional one might with advantage be appended to the excellent little chapter on affections of the teeth. Pages 99 and 134, exhibiting as they do respectively the causes of displacement forwards of the soft palate and of exophthalmos, may be taken as specimens of the care, knowledge, and research which characterise the whole book. The notice on page 60 of perforation of the septum nasi, occurring in workmen exposed to the vapour of bichromate of potash, shows how successful the author has been in keeping pace with the time. Tumours or swellings on the front or side of the neck form a long list; but we do not notice lipoma included. Yet it does occur in this situation, and its diagnosis from a polycystic hygroma is not easy. Post-pharyngeal abscess has often, when occurring in children, been mistaken for croup, and its giving

way under emetic treatment has been the first indication of the real condition.

Taken as a whole, the book is a valuable one, and the man who has it by him will find it again and again a trustworthy and handy aid to correct diagnosis.

On Syphilis. By V. CORNIL, Professor in the Faculty of Medicine of Paris. Translated, with Notes and Additions, by J. H. C. SIMES, M.D., and J. W. WHITE, M.D., Philadelphia. 8vo, pp. 461. London: Kimpton. 1882.

THE translation of Cornil's lectures on syphilis has been made by two American physicians. For the sake of uniformity they have changed the lectures into chapters, and they have added clinical information of their own to the extent of nearly one-third of the entire volume.

Cornil's opening definition of syphilis seems to us inferior in many ways to that of Mr. Hutchinson, and although it is admittedly difficult to reconcile the disease in all its manifestation and bearings with an exanthem, yet the theory as tested by clinical experience is a valuable and practical one. The translators strongly favour the lymphatic theory of syphilis, and give a very clear and able sketch of it, but in their summation of the matter come to the conclusion "that in all probability syphilis cannot be considered as a disease of either blood, lymphatics, or connective tissue, but rather as one involving commonly these and all other tissues." It is strange to learn from Cornil himself that, so late as 1861, Gibert, the Parisian dermatologist, taught that "syphilis followed gonorrhœa, vegetations, soft chancre as well as hard, and mucous patches; and to-day, as he records this, he declares himself a dualist of the school of Ricord and Fournier. In dealing with the question as to the chancre being a mere local development, not necessarily a symptom of general syphilis, and with its treatment by excision, the translators give fairly and succinctly the authorities pro and con. Their own conclusion derived from clinical experience is, on the whole, in its favour, and for this good reason, that in two cases at least in which the diagnosis was confirmed and in which the disease was derived from women with unquestionably active syphilis—one patient escaped entirely all constitutional harm, and in the other some slight glandular involvement was excited which speedily subsided. That the possible presence of the specific disease does not add to the risk of such operations Cornil's experience leads us to believe, for he closes some remarks "on the expediency of surgical operations during secondary syphilis," with these words: "for the minor operations, such as are performed in the Lourcine Hospital, we may report that we have never seen them followed by an

accident, and that they seem to us entirely safe." This testimony is of higher value when we read it in connexion with the remarks of Sir James Paget "on the risks of operation in syphilitic patients," for there we find this statement: "I am not sure that I ever operated on any one with active secondary syphilis." Cornil has done so repeatedly and without harm. With reference to the incubative period of the disease this rule is held as a good one—"that if an interval of ten days or more has elapsed between the last exposure to contagion and the development of the sore, the latter is probably the initial lesion of syphilis." The anatomical and histological characters of the hard and soft sore are admirably described, and that these are of practical moment is assured by the fact that ever and again the induration in an infecting sore may be absent, and the diagnosis depend greatly upon the character of the erosion, the condition of the surface of the chancre and of the surrounding lymphatics. The difficulty in diagnosis between a new indurated chancre, and an ulceration occupying the site of an old sore around which a certain amount of induration still exists, is pointed out, as is also the danger of reckoning the case one of re-infection, which arises when such ulcerations do occur.

The multiform cutaneous manifestations of the disease are dealt with histologically in a masterly way—as we should indeed expect them to be—and the accompanying illustrations are executed carefully and well. Fournier's dictum with reference to alopecia is of interest: he states "that persistent and general alopecia is never of syphilitic origin," and in dealing with this subject he points out an almost pathognomonic sign of syphilis most frequently seen in women,—the broken arch of the eyebrow resulting from the loss or thinning of the hair. Under the heading "Syphilis of the Muscles" is mentioned a syphilitic contraction of muscles chiefly seen in those of the arm, and more especially in the biceps. It appears in from six months to a year after the chancre—stiffening of the elbow, and difficulty in extension of the arm are complained of, and on the application of force the tension of the biceps resisting movement is at once evident. This tends to increase until the angle of flexion may vary from a large obtuse angle to one quite acute. The chief diagnostic sign seems to be "the strange unconquerable contraction of the biceps which produces an acute pain whenever forced extension is attempted; when examined during forced extension the muscle is found prominent, like a tightly drawn cord, the tendons particularly being hard and tense." That this condition is really of syphilitic origin is rendered probable by the observation of Mauriac, who found that in nine out of ten cases seen by him, there was a distinct history of syphilis.

The various nervous lesions, which are the recognised outcome

of the syphilitic dyscrasia, are treated with care and consideration. Syphilitic epilepsy, paralysis, cerebral syphilis, and locomotor ataxia, are subjects full of interest, and nowhere in the whole volume is the clinical experience of the author, or the wide acquaintance of the translators with medical literature, more evident than in Chapter X. The subject of tabes dorsalis and its association with syphilis is meantime a much disputed point, and although the matter is still a moot one, the teaching of this book is, as we gather it, rather in favour of a direct connexion.

In the bold stand they make for the recognition of "syphilis of the lung," the translators do well, for to us it seems from personal experience that difficult as it sometimes is to recognise the lesion solely by physical signs, there is a morbid pulmonary condition the favourable issue of which under specific treatment leaves little doubt as to its true nature. The last chapter, on the treatment of syphilis by mercury, is a fit close to the work. In its moderate and continuous use, author and translators alike find the best remedy for what we believe can only with danger be regarded as a self-eliminative disease.

The anatomy, the histology, the pathology, and the clinical features of syphilis are represented in this work in their best, most practical, and most instructive form; and no one will rise from its perusal without the feeling that his grasp of the wide and important subject on which it treats is a stronger and surer one.

Lehrbuch der Hautkrankheiten. By Dr. GUSTAV BEHREND. Second Edition. Berlin: 1883.

THE activity with which the study of diseases of the skin is now pursued is shown by the rapid succession of editions of good text-books. The first edition of the work under consideration was issued in 1879, and already a new edition is called for, which, improved in form and type, contains much matter not included in the first. Thus pharmacal, vaccinal, and menstrual eruptions are now fully treated of, and diseases of the nails, tubercle and various tumours of the skin, myxœdema, and certain newly-recognised parasitic affections, are systematically described. The text has also been revised throughout, and the most recent additions to our knowledge have been embodied, so that the work as it stands excellently represents modern dermatology.

It would not be possible for us, within the limits of the present review, to examine the work with any approach to completeness, nor is this necessary, as the first edition has long been known in England, and the general excellence of its contents would, we are sure, be admitted by all who have had occasion to use the book; but there are one or two points to which we may draw attention. We are met at the outset by

the question: "What constitutes a disease of the skin?" The author would probably answer, "Any local or general condition by which the skin is affected," and the same answer would, we think, be given by the majority of modern writers on dermatology. Accordingly we find that the book includes blushing, blueness from cold, measles, scarlatina, small-pox and vaccination, varicella, glanders, myxœdema, phlegmonous erysipelas, and ulcers; that is to say, physiological processes, numerous diseases (we have mentioned a few only) in which the affection of the skin is certainly to be looked upon as of secondary importance to more general pathological conditions, and diseases which are usually included in the domain of surgery. Now we do not deny the propriety of this view, but, if it be accepted, it becomes extremely difficult to decide what proportion to preserve between the various descriptions, and what affections to exclude altogether. Thus, in the work before us, we find measles, small-pox, &c., very fully described; while typhoid fever is only just mentioned under the head of roseola typhosa; and typhus exanthematicus, plague, and epidemic cerebrospinal meningitis, in which diseases the skin is more or less frequently and characteristically affected, are not noticed at all: again, glanders receives a careful description, while the commoner disease, pyæmia, in which a variety of skin affections are by no means uncommon, is only just referred to under the head of traumatic eruptions. Addison's disease is little more than named under bronzing of the skin, while myxœdema receives considerable attention; surely, if the latter is to be considered as a disease of the skin, there is no reason for excluding the various forms of jaundice and Bright's disease. We by no means intend to imply that Dr. Behrend is singular in his treatment of the subject; he has followed the common fashion; but it would be well, perhaps, for dermato-nosologists to adopt a more restricted scheme if the above inequalities are to be avoided: they are inevitable under the present plan.

The general characters of the work are fulness and accuracy, and the omissions are few and unimportant, but we think the author might, in a future edition, give some account of local asphyxia and symmetrical gangrene (*Raynaud*), included in his scheme of classification, but not further described. Indeed, the whole subject of gangrene of the skin and its antecedents (diabetes, &c.) seems to us to deserve a somewhat fuller treatment. Acrodynia, which has not been observed since 1829, might, we think, be omitted. In nomenclature, we are glad to find that Dr. Behrend writes leucoderm*i*a, scleroderm*i*a, &c., instead of the erroneous forms leucoderm*a*, &c., which are unfortunately current; but why does he admit the names ur*h*idrosis, chrom*h*idrosis, an*h*idrosis? The rough breathing is not preserved

in such compounds; we say uræmia, anæmia, not ur*h*æmia, an*h*æmia.

All these, however, are minor blemishes: the work remains one of the fullest and most trustworthy of modern text-books; and in some departments (especially vaccinal, menstrual, and pharmacal eruptions) contains the best accounts with which we are acquainted. We can confidently recommend it to all students of the important and interesting diseases of which it treats.

Clinic of the Month.

Poisoning from Swallowing Chloroform.—Oliver relates the case of a weakly man who swallowed ninety grammes (over three ounces) of chloroform. He was brought to the hospital at 11 P.M. The respiration was almost imperceptible, pulse very slow (twenty to the minute), feeble, and scarcely to be felt, skin cold and pale, face livid, pupils widely dilated, patient sleepy and anæsthetic. The breath smelt slightly of chloroform. Artificial respiration was at once begun. One pole of an induction-coil was placed over the heart, the other on the nape of the neck. Enemata of beef-tea with brandy, and subcutaneous injections of ether over the cardiac area were given. At 2 A.M. the skin was still cold, and showed no trace of sensibility. Five drops of nitrite of amyl were then given by inhalation, and at once the respiration began to improve. At 3 A.M. half a drop of nitrite of amyl dissolved in alcohol was injected beneath the skin, without any perceptible effect. At 5 o'clock, after six hours of artificial respiration, sensibility of the conjunctiva began to return, and the natural respiration became deeper. Gradually the skin grew warmer, and consciousness returned. In a few days the patient was well, complaining only of pain in the epigastrium, and of some pimples under the tongue. The urine contained neither albumen nor sugar. The absence of vomiting is notable; it was doubtless due to the local anæsthesia of the stomach and œsophagus. When the general anæsthesia was most profound the pupils were contracted; they dilated when the respiration had almost ceased. They contracted again on the application of the battery. Dr. Oliver was led to give nitrite of amyl by observing the general capillary spasm of the skin. (*Deut. med. Zeit.* Sept. 1882.)

Leucorrhœa in Children.—During a discussion on this subject at the Practitioners' Society, Dr. Kinnicutt said that he had met with a great number of cases of a very obstinate character in the New York Hospital. The leucorrhœa was not infrequently associated with the strumous diathesis, but occasionally careful

investigation had only revealed the presence of a large amount of uric acid in the urine. He suggested that the leucorrhœa might be due to the extension of a urethral inflammation, excited by this salt, to the vaginal mucous membrane. That a urethritis might be produced in this way there was no doubt; moreover, a vaginal catarrh was not of infrequent occurrence in gouty women. In the gouty diathesis it is well known that the mucous membranes throughout the body are prone to take on an inflammatory condition. In other words, there was present in the diathesis both an increased susceptibility of the mucous membranes and an irritant (uric acid) capable of acting locally. In the cases referred to there was marked irritation of the external parts.. Examination of the urine showed a very marked uric acid condition, occasionally almost amounting to "gravel." Dr. Kinnicutt cited several cases in which the leucorrhœa was pronounced, but of short duration. In his experience, the leucorrhœal discharges, associated with strumous diathesis, persisted for weeks, and even months. He would feel some doubt as to the propriety of local treatment in such cases. (*New York Med. Record*, Sept. 16, 1882.)

Diabetic Coma with Lipæmia.—A case of diabetic coma with lipæmia is reported by Drs. Fraser and Logan. On post-mortem examination there was found in the blood fatty matter, precipitated albumen, and a substance having an odour like that of acetone. The fat seems to have adhered largely to the sides of the blood-vessels, causing an obstruction to the flow along them, resulting in congestion and in extravasations of the vascular contents. The stasis thereby produced in the lung would account for the dyspnœa and cyanosis; and the cerebral congestion would probably cause coma, the production of which might have been aided by defective nutrition and by poisoning resulting from accumulation of effete matter and from the presence of acetone. These may be regarded as the immediate causes of the fatal termination. The remote cause seems to have been exposure to cold, one of the results of which was an inflammatory change in the kidneys, which interfered with their eliminative function, and hence the marked diminution in the quantity of urea, and probably of glucose excreted. The evidence appears to point to the fact that the fatty matter so largely present in the blood was universally and equally distributed throughout the body. Its origin in that case may be reasonably assigned to some constituent existing in the blood, which had somewhat rapidly undergone transformation. The blood-corpuscles could not have produced it, for their number was not lessened. The chemical relationship between glucose and fat is a very remote one, nor can albuminous matter be

either readily or easily transformed into fat. An altogether satisfactory explanation, therefore, of the origin of the abnormal constituents present in this and in similar cases cannot be given from existing data. We can only advance hypotheses; and the contribution of the authors to these hypotheses is—that from the glucose present in the blood the acetone was mainly derived, while the fatty matter originated from a transformation of the albuminous constituents of the blood-plasma. (*Edinburgh Medical Journal,* Sept. 1882.)

Plantago Lanceolata as a Styptic.—Professor Quinlan had an opportunity of observing the bleeding from leech-bites stopped by the application of the chewed leaves of the *Plantago lanceolata* by a cottager; and a number of trials in hospitals and elsewhere satisfied him as to the hæmostatic power of the plant, when applied to bleeding surfaces either in the chewed form or in that of the dried leaves. The plantain-juice does not contain tannin. When applied to the tail of a goldfish it does not interfere with the circulation in the web, but causes retardation almost amounting to stasis in the capillaries. He has used it with the very best results in cases of external hæmorrhage, and in cases of internal bleeding from the lungs, the kidneys, and the bowels; and in menorrhagia he has got fair results from large and repeated doses of the juice either fresh or mixed with alcohol or glycerine. (*Pharmaceutical Journal,* No. 637, 1882.)

Sudden Death in Gastric Ulcer from Entrance of Air into the Blood-vessels.—Jürgensen has recently met with two cases of this nature, one of which he describes with considerable detail. A woman aged 49 had for years suffered from the symptoms of gastric ulcer, particularly from repeated exhausting hæmorrhages from the stomach, by which she was reduced to the last degree of anæmia; she died suddenly. At the autopsy, an ulcer six to seven centimetres in diameter was found on the posterior wall of the stomach; the pancreas, duodenum, and left kidney were all adherent to the ulcer, on the surface of which there was found a large vein, probably the splenic, with a slit-like opening in its walls about one centimetre in length. The stomach was filled with clots: the veins of the liver and spleen were filled with air. Air was also found in the veins of the stomach, in the jugular veins, pericardium, heart-cavities, sub-serous cardiac tissue, and pulmonary blood-vessels. There was also sub-pleural and sub-intestino-peritoneal emphysema. (*Deutsches Arch. für klin. Med.,* Aug. 1882.)

Oil of Wintergreen in the Treatment of Acute Rheumatism.—Dr. F. P. Kinnicutt draws the following conclusions from the results obtained in twelve cases of acute

rheumatism, treated with oil of wintergreen. (1.) That in the oil of wintergreen we possess a most efficient salicylate for the treatment of acute rheumatism. (2.) That in its efficiency in controlling the pyrexia, the joint-pains, and the disease, it *at least* ranks with any of the salicyl compounds. (3.) That the best method of its administration is in frequently-repeated doses, continued in diminished doses throughout the convalescence. (4.) That its use possesses the advantages of being unattended with the occasional toxic effects, the frequent gastric disturbance, produced by the acid or its sodium salt, even when prepared from the oil of wintergreen; that its agreeable taste and finally its comparative cheapness are further recommendations in favour of its employment. (*New York Medical Record*, Nov. 4, 1882.)

Osteal and Periosteal Cachexia and Scurvy.—In the *Lancet* of July, 1882, p. 48, Dr. Cheadle points out the occasional supervention of symptoms of scurvy on a rachitic constitution. A child ten months old was suffering from the usual signs of rickets, with the following symptoms superadded : extreme cachexia, muscular debility, periosteal swellings, œdema of the legs, albuminuria, and hæmorrhagic spongy gums. This condition always results from errors in diet. In this case the child, when seven months old, was fed on arrowroot and isinglass for a month, and on Nestle's food for the three months immediately preceding treatment, milk being totally excluded. The treatment was simple antiscorbutic diet, without any medicine. This was followed by an almost immediate improvement of all the symptoms. Dr. Cheadle forms the conclusion from this case, amongst others, and a similar one reported by Dr. Gee, that, if Nestle's food were not the prime cause of the scorbutic condition, it was clearly powerless to remove it, and that this preparation alone is inadequate to supply the full elements of nutrition. (*London Med. Rec.* Nov. 1882.)

Trismus from Cerebral Lesion.—M. Lépine narrates the following case as a contribution to our knowledge of cortical localisation. The patient, a woman aged sixty-five, was brought to the hospital in a state of unconsciousness after an apoplectic seizure. After she had recovered consciousness she did not speak, the lips remained somewhat apart, the jaws were firmly clenched and could not be separated even by a considerable amount of force. There was no paralysis of the face or lips, no strabismus, the pupils were equal and sensitive, the movements of the eyes and eyelids were natural. There was flaccid hemiplegia of the left arm and leg, more complete in the arm than the leg. There was no loss of cutaneous sensibility. She died on the third day, the trismus having persisted almost to the last. On examination, a recent clot, about the size of a pigeon's egg, was

found on the right side just beneath the grey matter of the island of Reil and the foot of the ascending frontal convolution; the hæmorrhage had invaded and destroyed the *avant mur,* external capsule, and part of the outermost segment of the lenticular nucleus. There was also a yellowish cicatrix occupying the corresponding position in the left hemisphere, but somewhat smaller, and not extending into the *avant mur,* being confined to the external capsule and lenticular nucleus. M. Lépine points out that the situation of this recent hæmorrhage corresponds exactly to the spot determined by Ferrier's experiments to be the centre for the movements of the muscles of mastication. (*Revue de Médecine,* Oct. 1882, and *Med. Times and Gaz.*)

The Treatment of some Chronic Gastric Disorders.—M. Broca, in a series of articles, strongly advocates, in ulcers of the stomach and chronic gastritis from various causes, the systematic washing-out of the stomach and artificial feeding. In washing out the stomach there are two indications to fulfil—one, to empty it of its contents, whatever they may be; and the other, to treat the diseased mucous membranes with medicated solutions. He advocates the use of the siphon-tube, on the ground that its manipulation is so easy that in a very short time the patient can learn to wash his stomach out himself. The washing over, the patient should be fed before the tube is withdrawn, powdered meat, raw eggs, milk, or broth being the most suitable food. He lays great stress on the advantages of over-feeding the patient, and mentions 600 grammes of raw meat, a dozen eggs, and three litres of milk as a daily allowance that may easily be exceeded. It is always necessary to commence gradually, until it is ascertained that the patient can digest milk and eggs well. Should pain come on some hours after the meal, it is advisable to empty the stomach by means of the tube. The cure is permanent if the patients only take proper care of themselves afterwards. He thinks that this mode of treatment might with advantage be extended to other than purely gastric disorders; he suggests it, for instance, in advanced phthisis. (*Le Progrès Médical,* 39-42, 1882.)

Syphilitic Polyuria.—There is, according to Professor Semmola, of Naples, a form of cerebral syphilis which may be the cause of polyuria. The *Revista de Ciencias Médicas* of Barcelona speaks of three cases already reported by the Professor in support of his opinion. In one of those cases (the most characteristic of all) the patient used to void forty-three pints of urine in twenty-four hours, with a specific gravity varying between 1001 and 1005. He had seen several physicians, but, feeling no relief from their treatment, he finally consulted Professor Semmola, who found out that the man was affected with chronic

syphilis, to which he attributed the causation of his disease. Very probably some syphilitic lymph or deposit was locally effused into the walls of the fourth ventricle of the brain, and so had pathologically reproduced the celebrated physiological experiment of Claude Bernard—that is, the production of polyuria and glycosuria in dogs by simply puncturing with a needle the floor of the fourth ventricle. In accordance with this diagnosis, the patient was submitted to a general antisyphilitic treatment, which consisted in hypodermic injections of albuminate of mercury and the long-continued use of iodide of potassium. In two months he was perfectly cured. (*Philad. Med. Times*, Nov. 4, 1882.)

Cure of Hydatid Cysts by Capillary Puncture.—Dr. Alessandro Borgherini reports *in extenso* the histories of four cases of echinococcus cysts treated by capillary puncture and withdrawal of a small quantity of fluid. Of the four cases three were cured, but in the other a second puncture with complete evacuation of the cyst was necessary. The punctures were made with the needle of a hypodermic syringe, and the amount of fluid withdrawn was from one-half to two drachms. A slight elevation of temperature followed the operation in every instance, but in one case only did the fever continue for any length of time or rise to any considerable height. Improvement did not follow until from eight to fifteen days after the punctures were made. The author thinks that possibly the cure is brought about by the altered tension caused by the abstraction of a small amount of fluid and the consequent disturbance of osmosis, a process by which the parasite obtains nourishment. Or possibly the slight puncture acts as a traumatic injury impairing the vitality of the parasite. (*Gazzetta Medica Italiana*, Aug. 5 and 12, 1882.)

The Action of Sodium Salicylate on the Heart.—Prof. Maragliano, of Genoa, has investigated this subject afresh, the results of previous observers being in many points contradictory. Some make out that the heart is weakened, others that it is strengthened by the salicylate. Liebermeister, for instance, in Zeimssen's Cyclopædia (vol. i.), says he never gives the salt to a patient whose heart is weak, and he advises that it should never be prescribed in such cases. Maragliano made a triple series of observations to determine the question. In the first, patients who were taking the salicylate regularly were examined sphygmographically before, during, and after the exhibition of the dose, morning and evening. In the second, the pulse-curve of other persons was taken, before and after the exhibition of a single five-gramme dose. In the third, the arterial pressure was measured by Basch's method before and after the exhibition

of a single five-gramme dose. The results showed that—(1) As the dose was gradually increased in patients of the first class, the pulse became progressively stronger, and the systolic line of the sphygmographic trace became higher. (2) After the single dose the pulse was stronger and the systolic line higher; the increase appeared an hour after the dose, reached its maximum in two to three hours, and disappeared after three to five hours; the normal dicrotism was accentuated, and often passed into tricrotism. (3) The arterial pressure rose about an hour after the exhibition of a single dose, and returned to the normal about three hours afterwards. The rise in pressure varied between ten and twenty millimetres of mercury. These results indicate unmistakably that no depressing influence is exerted on the heart by the salicylate. (*Centralb. f. med. Wiss.*, Dec. 2, 1882.)

Syphilitic Enlargement of the Tonsils.—An abstract of the conclusions of Dr. Paul Hamonic is to be found in the *Deutsche med. Zeitung*, No. 45. Hamonic distinguishes, during the secondary stage of syphilis—(1) *Simple hypertrophy*, which is analogous to the swelling of lymphatic glands, is tardy in its development, and, as it occasions no symptoms, is often overlooked. Both tonsils are almost always affected, though to a different degree. The enlargement takes place forwards, bulging the anterior pillar of the fauces, and rarely gives rise to deafness. The tonsils are hard and somewhat elastic; the normal depressions on their surface are exaggerated. The uvula tends to go over to the large tonsil. Sometimes the tonsil may be reduced in size by anti-syphilitic treatment. (2) *Hypertrophy associated with angina.* In this there is not so much fever as in ordinary acute angina; the duration is variable, and relapses are very liable to happen. (3) *Hypertrophy complicated with syphilides.* Most frequently the syphilide appears on the tonsil and the anterior pillar of the fauces. When syphilis affects a previously scrofulous tonsil the enlargement is very great, of pale colour, often spongy and with large crypts, there is considerable pain, the voice becomes nasal, and the hearing, taste, and smell are altered. The course is generally chronic, and there is a great tendency to recurrence. Ordinary tonsillitis and sore throat may supervene even when the tonsils are enlarged from syphilis, but then, though peritonsillar suppuration may occur, it would appear that the tonsil itself never suppurates. Hamonic states that there is no objection to excision of the syphilitic tonsils if they be very large. (*Med. Times and Gaz. Dec.* 2, 1882.)

Trigeminal Neuralgia.—Dr. Vandenabeele reports that in Dr. Rothery's practice at the Hôtel Dieu the treatment of this affection by ammoniacal sulphate of copper, as recommended by M. Féréol, has been found useful. M. Féréol employed it in

obstinate neuralgias which had resisted numerous other measures, and presented all the characteristics of facial *tic douloureux.* In almost all cases the sulphate relieved the pain, sometimes instantaneously, and restored sleep to the patient, who had been deprived of it for weeks. The usual dose was from ten to fifteen centigrammes, which was increased to thirty or even fifty, according to the susceptibility of the patient. The only inconvenience produced by these large doses arose from disturbance of the digestive organs, which ceased as soon as the medicine was suspended. M. Féréol also prescribed ammoniacal sulphate of quinine, ordering from ten to fifteen centigrammes with thirty of syrup of orange-flowers, a hundred grammes of distilled water, and giving a tablespoonful at meal-times. The medicine should be continued for a fortnight, and its action is more decided in proportion to the amount of congestion that exists. (*Jour. de Thérap.* Oct. 25, 1882.)

Deafness following Mumps.—Dr. Johannes Seitz reports a case of deafness following an attack of parotitis, only five other cases having been hitherto recorded. The patient, a student, nineteen years of age, was seized with inflammation of both parotid glands, of considerable intensity, but unaccompanied with any cerebral symptoms. On the sixth day the patient seemed well. Two days later he returned, complaining of deafness on the right side with tinnitus aurium and dizziness. The watch could be heard only when in contact with the head. The drum of the right ear was thinned, opaque and slightly sunken, yet the light pyramid appeared normal. On the left side there were evidences of old middle-ear catarrh, but no signs of recent inflammation. There was no complaint of the left ear except for a day or two, when a sound like the roaring of the wind was present. This soon subsided and no further trouble was experienced on this side. The dizziness and tinnitus continued for two or three weeks and then gradually ceased, but the loss of hearing on the right side remained permanent. (*Correspondenz-Blatt für Schweizer Aerzte*, Oct. 1, 1882.)

Herpes Labialis in Malarial Fever.—Messrs. Verneuil and Merklen, after a careful study of the facts connected with the eruption of *herpes facialis* in febrile affections, have come to the conclusion that when it accompanies malarial fever it has no special characters. In its ordinary and unimportant forms it may be preceded and accompanied by vaso-motor derangement and altered sensibility of the skin at the affected part. The authors suppose that the affection may be produced by congestion of the cutaneous nerve twigs, resulting from a localised effect of the malarial poison. (*Annales de Dermatologie et de Syphiligraphie*, vol. iii. No. 11.)

Extracts from British and Foreign Journals.

Effects of Iron on Digestion.—In an inaugural dissertation published at Berlin, Dr. A. Düsterhoff records the results of some experiments bearing on this subject. One gramme of fibrin was added to 20 ccm. of artificial gastric juice, and during digestion equivalent quantities of various preparations of iron were also added. At the end of the process the undigested fibrin was dried and weighed, and the quantity of soluble syntonin in the solution was also estimated. The time of digestion was in one case 3h. 10m.; in another it was 7h. 30m. In the first series 0·0614 gramme of metallic iron was in each case added, in the form of pyrophosphate, perchloride, and protolactate respectively. In the second series, various other preparations were used, the amount of metallic iron being in each case equivalent to 0·0077 gramme. Other experiments were made with white of egg; the amount of albumen precipitated by boiling after digestion being estimated. The outcome of the experiment is—that the organic salts of iron seriously hinder and check peptic digestion. Probably the hydrochloric acid of the gastric juice displaces the organic acids from the iron salts and so is used up; while the free organic acids in the digestive fluids are far less powerful digestive agents than the hydrochloric acid. But this cannot be the only cause at work, for perchloride and phosphate also tend to hinder digestion. Even reduced iron has a similar effect, for it partially dissolves in the juices, forming chlorides. Its solubility, like that of the phosphate, is however not very great. Ferrous salts seem to interfere less with digestion than ferric salts. (*Centralb. f. med. Wiss.*, Nov. 11, 1882.)

Binaural *versus* **Uniaural Stethoscopes.**—[Dr. Malet, of Wolverhampton, read the following highly practical paper at the Worcester meeting of the British Medical Association. As we have heard frequent complaints of the difficulty of making good observations with the binaural stethoscope, we have no hesitation in giving the paper entire, and commend it to the attention of our readers.]

That so unquestionably superior an instrument as the binaural stethoscope should not be more universally adopted, is a matter that calls for explanation; and this is, I think, to be found in ignorance of the minute manipulative details necessary in using it. In these it essentially differs from the uniaural; and as each one has to find out the peculiarities for himself, he is apt to conclude that the deficiency which he naturally experiences is in the instrument rather than in his method of using it—an error which it will be seen is all the more likely to occur if he be already skilled in the use of its rival. I have thought it worth while, therefore, to give a short account of the precautions necessary in using the binaural stethoscope; and in furtherance of this subject, to make a few remarks on the physical difference in the conduction of sound by it and the uniaural.

The form of binaural stethoscope to which I refer is that usually known as Andrew Clark's, consisting of a rigid hollow chest-piece, from which pass two flexible tubes, each terminating in a rigid tube and ear-piece accurately adjusted to each auditory meatus.

In the ordinary uniaural stethoscope, the sound is almost altogether conducted by the substance of the instrument, as is shown by the use of solid stethoscopes, and the little difference produced by plugging a perforated one; by this solid medium, the vibrations of a considerable portion of the chest-wall round where it is firmly resting are conducted to the head of the auscultator, and it follows that we hear with it the sounds from a considerable portion of the chest at one time; moreover, the sounds are somewhat intensified by firm pressure, which renders the solid continuity more perfect. With the binaural stethoscope, exactly the reverse is the case, the intervention of the flexible tubes destroys the solid continuity of the instrument, and, therefore, conduction through its substance; the sound is almost wholly conveyed by the air within the tubes, and an accurately fitting plug in the chest-piece very greatly diminishes it, what remains being conveyed by the wood of the chest-piece past the plug to the air beyond it, and by that to the ears. It follows that almost the only vibrations conducted to the ears are those from the drum of skin immediately beneath the chest-piece, the rim of which tends to cut off the vibrations of the surrounding chest-wall from this skin-drum, and so from the air within the stethoscope. This method of conduction by the binaural necessitates the following precautions in its use.

1. Perfectly accurate adjustment of the chest-piece to the skin is absolutely necessary; the slightest communication between the external air and that within the stethoscope not only admits a confusion of foreign sounds, but prevents the conduction of the vibrations of the skin-drum to the ears. This adjustment is

sometimes difficult in the examination of thin chests, with narrow intercostal spaces; it will be much facilitated by covering the end of the chest-piece with tolerably thick india-rubber. Of course it is almost equally necessary that the ear-pieces should perfectly fit the auditory meatus; but in this there is not likely to be any errors. With the uniaural, all that is necessary is that the stethoscope should rest firmly between the ear and the part examined.

2. It is necessary to be cautious in the pressure exercised on the skin. If there is much subcutaneous fat, any excessive pressure fixes the drum of skin under the chest-piece against the tissues beneath it, and checks its vibrations. In some cases this effect is very marked, and may materially interfere with the delicacy of an examination. As a rule, the best pressure is that only just sufficient to keep the rim of the chest-piece in complete contact with the skin. In thin patients, when listening to the neck or abdomen, pressure may intensify sound by rendering the skin-drum more tense. With the uniaural stethoscope, on the contrary, fairly firm pressure always renders the conduction more perfect.

3. On account of the light pressure necessary in using the binaural, there is a liability to friction occurring between the chest-piece and a dry or a hairy skin; and this is exaggerated in common with other sounds by the use of both ears; the firm pressure of the uniaural obviates this annoyance. It may be effectually avoided in the binaural by using the india-rubber covering for the chest-plate alluded to above, against which the hair and skin will not slip.

4. On account of the very limited area from which sounds are conveyed by the binaural, it is necessary to examine a patient very universally, or some abnormal sound may be missed through its not being under, or very near, the chest-piece. As regards localisation and intensity, the two stethoscopes may be happily compared to the low and high powers of a microscope, the binaural having a smaller field, but higher power. The ease with which the binaural can be used, renders such complete examination less irksome to both patient and auscultator, than a more curtailed one with the uniaural would be.

5. The muscular *bruit* is heard with such distinctness through the binaural stethoscope, that it will be found always very advantageous, and when making a very minute examination, quite necessary, to have the patient in a position of complete rest. If, for instance, when examining a recumbent patient, the head is not properly supported, the hum of the cervical muscles may render minute intrathoracic sounds quite obscure.

With the exception of the last, these binaural precautions are seen to be more or less directly the reverse of what are

advantageous in the uniaural; and it readily appears how a skilled uniaural auscultator, adopting his usual method when first trying a binaural stethoscope, would undervalue the instrument. The binaural stethoscope is not without real disadvantages; and of these, I believe the following to be the only ones worth mentioning. In some few cases, where it is necessary to make considerable pressure to get near the object we wish to listen to, the binaural is defective, on account of the light touch necessary in using it; for instance, when listening in fat subjects to intra-abdominal sounds, the pressure necessary to approach the deeper parts of the abdomen interferes, as explained above, with the vibrations of the skin-drum under the chest-piece, and enfeebles the sounds; this is very marked in auscultation of the uterus.

A slight disadvantage also arises from the necessity of applying the binaural directly to the skin, the intervention of even the least clothing both checks the vibrations of the skin-drum, and admits extraneous sounds into the chest-piece, and it is, in consequence, necessary to strip a patient for the most superficial examination; with the uniaural stethoscope and sufficient pressure, any intervening clothes are so compressed as to be practically a continuous solid with the stethoscope, and hence a rough examination may be made through some clothing—a very questionable advantage. I have also noticed that in some cases of consolidation of lung, especially in children, bronchial breathing was more marked with the uniaural than with the binaural stethoscope, due, doubtless, to the solid lung forming, with the uniaural, a less interrupted conductor than with the air in the binaural, and to the sound being heard from a larger portion of chest at once with the former. It should also be borne in mind that if there is any defect in the membrana tympani or auditory canals of the auscultator, the uniaural stethoscope will, for all purposes, be the better instrument, as its vibrations are communicated to the bones of the head, whereas the binaural relies altogether on perfect conduction by the air in the stethoscope to the membrana tympani.

But these disadvantages are so slight as not to weigh in my mind for a moment against the superiority for general purposes of a properly used binaural stethoscope; in the first place, we have the infinitely greater comfort to both patient and examiner, the light touch of the chest-piece, the readiness with which (when the flexible tubes are of a proper length) a recumbent patient is ausculted without being much disturbed, while the examiner is saved from strained and uncomfortable attitudes, and is so much the more capable of arriving at a deliberate and correct conclusion. Again, there is the rapidity with which a very complete auscultation can be made, and, in particular, the

great ease with which children can be thoroughly examined. The greater distinctness with which sounds are heard through the use of both ears, and also the concentration of attention obtained by the exclusion of all external noises, are marked advantages over the uniaural stethoscope. The very great accuracy with which the binaural localises sounds, is frequently of much service. The light touch with which the binaural is best used renders it the only instrument suitable for the auscultation of venous and similar bruits, in which case also the advantage of seeing exactly what we are doing, at all times very important, is also marked. I would venture, in conclusion, to point out that it is necessary, when first acquiring the use of the binaural stethoscope, to supplement it with the accustomed uniaural, as all sounds are sufficiently different with the former to render their recognition uncertain at first. (*Brit. Med. Journ.*, Oct. 21, 1882.)

The Chemistry of cute Yellow Atrophy.—In a case of this disease Salkowski estimated the quantity of peptone and hemi-albumose in the liver, spleen, and kidneys. In normal conditions these substances are either absent altogether or only present in traces in the organs named. In this case the amount of peptone and hemi-albumose in the liver were respectively 3·57 and 0·71 per cent.; in the spleen 3·40 and 0·95; in the kidneys 2·56 and 0·39. The great resemblance between the decomposition of albumen in the organs in this disease and digestion with trypsin led him to search for tryptic ferment in the liver. By extracting with glycerine, however, he got no definite result; the glycerine extract dissolving fibrine but only after several hours and incompletely. In regard to the hemi-albumose, he found that Kühne was right in his statement that this body is insoluble in water. His own previous statement that it is soluble in water he now finds to be due to a slight error; the specimen he used having contained a small quantity of acetic acid. (*Virchow's Archiv*, Vol. 88.)

Ocular Changes in Hepatic Disease.—Hæmorrhages into the retina have been found in catarrhal jaundice, gall-stones, carcinoma, cirrhosis, abscess, acute atrophy, phosphorus poisoning, dropsy of the gall-bladder, and in pneumonia accompanied by icterus. Such extravasations are by no means always of bad omen, seeing that they may accompany a harmless catarrh, provided this be attended by jaundice. In a case of acute yellow atrophy, due to phosphorus poisoning, Litten (*Wiener med. Woch.*, No. 39) found, by the side of multiple fresh hæmorrhages into both retinæ, certain white areas situate in the granular layer, which microscopically showed fatty degeneration with

tufts of tyrosin and granular spheres (? of leucin). The blood-capillaries had undergone marked fatty metamorphosis. In two cases of atrophic cirrhosis, Litten observed simultaneous pigmentary degeneration of the retina, which in one instance had developed after the liver disease had already been of long standing, whilst in the other case it was found in the course of the first year. The formation of pigment may extend very rapidly. Nyctalopia (night-blindness) sometimes accompanied a hypertrophic or atrophic hepatic cirrhosis without any sign of organic change in the structures of the eye. Immediately after the tapping of a considerable ascites, a neuro-retinitis developed itself, with slight swelling of the optic disc and exudation into the tissues and around the vessels. (*Med. Times and Gazette*, Oct. 14, 1882.)

Excretion of Alkaline Salts and Urea during Convalescence.—Salkowski finds that during convalescence from typhoid a retention of albuminous matter occurs in the body, in order to make up for the waste which has taken place during the fever. This retention is shown by the fact that the amount of urea excreted is much less than corresponds to the quantity of nitrogenous food eaten. The retention of albumen and diminution of urea appears to begin about the seventh day; but it increases very greatly about the fourteenth day after the temperature has become normal. The patient in question was not long enough under observation to ascertain how long this retention of albumen lasted. A few days after the last rise of temperature was noted, the excretion of potassium lessened to an extraordinary extent; in ten days afterwards it again rose, and then remained constant. In these ten days it hardly amounted to 1-6th of the normal quantity, and was much less than that of a patient taking almost no food immediately after the crisis of acute pneumonia. It is very remarkable that the retention of potassium in the body by no means runs parallel with the retention of nitrogen, and that the latter occurs to the greatest extent after the former has nearly ceased. The tissues in fever must either become very much poorer in potassium salts (which is very unlikely) or during convalescence an accumulation of potassium-salts must occur before the regeneration of tissue begins to any great extent. (*Virchow's Archiv*, Vol. 88.)

Synthesis of Uric Acid.—It is stated that Dr. Horbatschewsky, of the Vienna Chemical Institute, has succeeded in forming uric acid synthetically. As is well known, all attempts to produce this substance artificially have hitherto failed, and considerable doubt has existed with regard to its exact constitution, though it is generally represented as consisting of one radical of tartaric acid and two of urea. From

the fact that uric acid under powerful oxidation splits up into molecules of urea, it has been assumed that this body is one of the substances through which every particle of albumen passes before it is thrown out of the body, and on this assumption it has been taught that when oxidation is imperfectly performed there is an accumulation of insoluble uric acid in the blood, which replaces some portion of the urea which ought to be formed. It is upon this view that the doctrine of *lithæmia* has been founded. On the other hand, there are a few who believe that uric acid in the human body in health, and even in disease, is formed in only very minute quantities, and that its pathological importance is to be referred rather to its insolubility than to its excessive production in the system. They hold that though uric acid contains residues of urea, it is not necessarily an antecedent of the latter, and that it is more probable they both start from a body containing at least some of its nitrogen in the form of cyanogen, and that the final cause of divergence lies in the fact that urea is the form best adapted to a fluid excrement, as in the case of mammalia, and uric acid to a solid excrement, such as is met with in birds and reptiles. This view is strengthened by the increasing evidence we have that the chief antecedents of the urea in the blood are partly the kreatin formed in muscle and elsewhere, and partly the leucin and other like bodies formed in the alimentary canal. A writer, who has recently investigated the question from its clinical aspects, remarks that if this view be accepted, uric acid will be regarded as a consequence and not a cause of the manifold disorders to which it has been said to give rise; that when it is deposited, the fact of the occurrence of the deposit will have to be referred to the insolubility of the minute quantity that exists in human blood, rather than to any excessive production in the system, and that attention will then be primarily directed to the discovery of the circumstances which lead to the deposit of this insoluble substance, rather than to vague generalisations concerning its over-production from hepatic derangement or gouty proclivities. (*Lancet*, Nov. 18, 1882.)

Origin of the *Bothriocephalus latus*.—Having succeeded in demonstrating the presence of scolices of the bothriocephalus in the muscles, the liver, and organs of generation of the pike and other animals, Braun (*St. Petersburg med. Woch.*, No. 16, 1882), endeavoured to breed a tape-worm out of the healthy scolices in the intestine of a mammal. He succeeded beyond his hopes, in the case of cats and dogs; and proved conclusively that the worm found in their intestines, after appropriate feeding, differed in no particular from the bothrio-

cephalus of man beyond being of a smaller calibre, corresponding to the altered situs. This, then, is a solution of the hitherto vexed question as to the origin of the parasite; the source from which it springs being the pike, as the source of the tænia solium is the pig. Medium-sized pikes may contain forty or fifty of these worms, and larger ones more. In sixty pikes examined for them, there was only one in which traces of the bothriocephalus could not be found in the muscles. (*London Med. Record*, Nov. 1882.)

Alterations in Cane Sugar in the Human Stomach.—Professor Leube finds that the gastric juice secreted both by healthy human stomachs, and also by certain diseased ones, has the power of converting cane sugar into grape sugar, or at least into a sugar having the power of reducing copper. This grape sugar after its formation is absorbed by the healthy stomach, but not by the stomach affected with dilatation. The conversion of cane into grape sugar is chiefly effected by the gastric juice, the action of the mucus being apparently unimportant. (*Virchow's Archiv*, Vol. 88.)

Carbonate of Ammonium as a Stimulant.—Dr. E. P. Brewer, of Norwich, Ct., publishes the results of a series of experiments undertaken for the discovery of the essential nature of the stimulant power of carbonate of ammonium. He declares, as a result of his labours, the belief that "the action of carbonate of ammonium is not due to the presence of the carbonic acid in combination with the base, but is dependent on the absorption of free ammonia while the salt is chemically combining with the hydrochloric acid of the gastric juice. The instability of the compound which renders it so susceptible of digestion is the quality that ranks it above all other ammoniacal salts." The practical deduction is that carbonate of ammonium is only serviceable as a stimulant for cases in which the secretion of gastric juice is but slightly affected. This is true of many acute maladies. In graver cases, when gastric digestion is deficient, the value of the remedy is correspondingly lessened. In chronic cases, and in acute cases of long duration, such as the latter stages of fevers, it is practically valueless. Unlike alcohol, it possesses no nutrient properties. (*American Journal of the Medical Sciences*, July, 1882.)

Helenin in Tuberculosis.—In a recent communication to the Société de Biologie, M. de Korab described some results following the use of helenin in pulmonary diseases. The same observer reports (*Comptes Rend.*, vol. xcv.) some experiments made as to the action of that substance upon the bacilli of tuberculosis. He states that, when the organisms were sus-

pended in sterilised serum and placed in tubes, into some of which helenin was also introduced, the bacilli multiplied in the liquor containing no helenin, but that in which it was present showed no signs of their development. Further, whilst the former liquid, when injected into animals, produced the tuberculous condition, the latter appeared to be inert in this respect. Some other experiments appeared to show that helenin, administered in the food or injected subcutaneously, acted as a preventive to tuberculous infection by inoculation, or in cases where the disease already existed, modified it favourably. Helenin is a crystal-like substance occurring in small quantity in elecampane root (*Inula helenium*), and is represented by the formula C_6H_8O. (*London Med. Record*, Nov. 1882.)

Treatment of Sequelæ of Frost-bite.—Dr. Lapatin, of Tiflis, communicated to the Caucasian Medical Society his discovery of an effectual means for the relief of a very annoying sequela of the milder degrees of frost-bite, in the form of pains and a very troublesome prickling sensation in the parts, which come on for years afterwards, especially in the colder seasons and on sudden changes of weather. After trying all the remedies which had been recommended, he found that the best was an old one called" Rust's frozen wash," consisting of equal parts of dilute nitric acid and peppermint-water. With this the parts are brushed over once or twice a day, preferably by means of a glass brush. After three or four applications the skin assumes a brown colour and becomes dry, and a superficial scab forms, which when thrown off leaves a healthy skin. Within one and a half or two weeks the sensations, which frequently prevent frost-bitten soldiers from putting their boots on, disappear for ever. The writer desires to make known a far better, quicker, and more reliable remedy, which never fails if mortification has not set in, viz., balsam of copaiba. This is spread thickly on a piece of muslin or linen, and the affected parts covered with it during the night, a stocking being put over the whole. In the daytime some of the balsam is merely spread over the parts. After one or two applications the redness and pains cease, and a few more not only remove every sequela, but seem to impart to the surface a remarkably increased resistance against frost-bite, if common precautions be used. The writer has long considered this remedy an unfailing specific. (*Philadelphia Med. Reporter*, Oct. 7, 1882.)

Action of Oxygenated Water on the Animal Organism.—Colasanti and Capranica have made a series of experiments on dogs with oxygenated water, H_2O_2, and find that when made to absorb it they experience toxic effects and rapidly die. Toxic doses vary with the size of the animal, 25 ccm. proving fatal to

a dog weighing three kilos, or six and a half pounds, and 75 ccm. to a dog weighing thirteen kilos. The intoxication manifests itself on all the functions of the body, but especially on those of the spinal cord. The excito-motor power of this centre is over-excited, the manifestations of which are convulsive phenomena, tetanus, and locomotor ataxy. The physico-chemical phenomena of the tissues are also profoundly modified, for it produces strong glycosuria before death. The disorders of the economy result from the decomposition of the oxygenated water in contact with the living tissues. The phenomena consecutive upon intoxication with oxygenated water are identical with those observed by Paul Bert following the action of compressed oxygen. (*Arch. Italiennes de Biologie*, vol. ii., and *Lancet.*)

Salts of Manganese.—The action of certain little-studied salts of manganese, that form compounds with molybdic and tungstic acids, has been investigated by Merti and Lüchsinger. The same symptoms were produced in various animals experimented on. In all a depression of all the functions of the nervous system was produced. Somnolence was succeeded by diminution in reflex action, lessened respiratory movements, and lowered blood-pressure. As the paralysis increases the heart beats with increasing frequency and force. With these phenomena there is a gradual fall in temperature and in the production of heat. In warm-blooded animals there is also considerable irritation of the intestinal tract, apparently connected with the elimination of the salts. Diarrhœa even follows their hypodermic injection, and vomiting occurs early in those animals which are disposed to it; and when the poisons are injected under the skin, even the first vomit shows traces of their presence. (*Centralb. für die med. Wissenschaften*, 38, 1882.)

The Treatment of Locomotor Ataxy.—Professor Hardy thus summarises the treatment of this affection, at the end of a lecture reproduced in the *Gazette des Hôpitaux.* "The chapter of treatment is unfortunately but a very short one; for, as a rule, whatever we may do, ataxy follows a progressive course. However, there are some cases in which not only is it possible to relieve the patient, but also to delay the progress of the disease. The treatment which succeeds best, and which is generally advised, is that which has for its base iodide of potassium. Nitrate of silver is likewise a good means, employed concurrently with iodide of potassium, although it has not always fulfilled the promise first given by it. Therefore, I give during fifteen consecutive days two grammes of iodide of potassium, then during the fifteen following days two or three centigrammes of nitrate of silver in pills, each containing one

centigramme. Finally, recourse is had to rubefacients; blisters, cauteries, applications of tincture of iodine on the sides of the vertebral column, and especially actual cauterisations, to the number of ten or fifteen, repeated every eight or ten days. These are not very painful. Sulphurous baths are likewise recommended. For the lightning pains, extract of belladonna, sulphate of quinine, salicylate of sodium, in doses of from three to five grammes, and hypodermic injections, are prescribed. Hydrotherapy has been vaunted for ataxic cases; but I have not been able to discover that any benefit has been derived from it. I prefer mineral waters, slightly mineralised, but alkaline and warm, such as those of Neris, Plombières, Lamalon, etc. The latter especially tend to diminish the pains, to improve movement, and sometimes even to retard the progress of the disease. Such are the means pursued, not to cure locomotor ataxy, but to retard its progress, and to give relief to the patients who are attacked by it." (*London Med. Record*, Nov. 1882.)

Ptomaines in Arsenical Poisoning.—In arsenical poisoning, the body decomposes slowly, and ptomaines (or cadaveric alkaloids) are therefore peculiarly likely to be formed. It appears that these, sometimes at least, contain arsenic, and Selmi and Ciaccio found that one volatile ptomaine resembled strychnine in its action, while another non-volatile ptomaine produced torpor, paralysis, and systolic arrest of the heart. Both of them contained arsenic and were intensely poisonous, more so than arsenic itself. It appears probable that these ptomaines were the active principle of certain secret poisons used in the middle ages, the "acqua Toffana," and the so-called "acquetta di Perugia." A secret compound of the poison-mixers of the 17th and 18th centuries was, according to tradition, prepared by killing a hog, disjointing the same, strewing the pieces with white arsenic which was afterwards rubbed in, and collecting the arsenical liquid which dropped therefrom. A liquid obtained in this way was said to possess a much more poisonous action than simple solution of arsenious acid. It was probably for a similar object that the juice of a plant was added to arsenic in the preparation of acqua Toffana; the juice of *Linaria cymbalaria* was usually employed.

Selmi and Vella presume that through the acquetta di Perugia the concealment of the action of the arsenic on the one hand, and of the tetanic poisons on the other, was accomplished. This supposition, which is based on an observation of Vella in a case of complex poisoning with arsenic and strychnine, does not agree perfectly with experiments on warm-blooded animals. In them, through the simultaneous application of arsenite of potassium and strychnine, the tetanic spasms were

not prevented, provided that the strychnine was given in a toxic dose. From still another point of view the ptomaines containing arsenic appear to be of significance in toxicology, from the fact that they help to explain an affection, that of chronic arsenical poisoning, produced by arsenical wall-papers. As Selmi has shown that a volatile arsine is formed by the contact of arsenious acid and albuminous matters, which possesses a strongly toxic action differing somewhat from that of arsenious acid, the author thinks it may be presumed that a similar product may be formed from the glue which is employed for affixing the arsenical wall-paper of a room. The moisture of the air may perhaps also play a part in the formation of the arsine. (*Pharmaceutical Journal*, No. 625, 1882.)

The Relation of the Diapedesis of Leucocytes to Iodoform.—Binz has investigated the action of iodoform on leucocytes in the mesentery of the frog, in the same way as he has previously done with quinine and eucalyptus. The mesentery is placed on a ring of cork in such a way that the intestine surrounds it like a wall and prevents the iodoform, which is dissolved in almond-oil, from flowing away. The solution of iodoform in almond-oil is made without the aid of heat, and in a dim light, in order that the iodoform may not be decomposed ; and during the whole experiment the frog is kept in diffused daylight. The action of the drug is tested either by placing two drops of the solution of iodoform on the mesentery in the morning, removing it at night, and examining the mesentery; or by repeating this experiment with a mesentery in which the diapedesis is going on briskly, but before the leucocytes have got beyond the outside of the blood vessels.

Binz finds that iodoform prevents the leucocytes from passing out through the vessel, and causes those that have already passed out to remain quiet without wandering further through the tissues. No alteration occurs in the capillary circulation ; the beats of the heart and the volume of the vessels are unchanged; and no alteration can be discovered in the wall of the vessels by the strongest magnifying powers. Almond-oil alone without iodoform has some effect in preventing the diapedesis, but it is not nearly so great as when iodoform is dissolved in it. When the vapour of iodoform is applied to leucocytes in a moist chamber, their power of motion is soon destroyed, and in a short time they die. Under the influence of daylight iodoform is decomposed and free iodine given off; this volatilises, passes through the thin wall on which the leucocytes lie, and paralyses them. When the development of iodine is too rapid the action of the iodine extends first to the

column of blood in the capillaries, where it causes coagulation; next it renders the heart weak, and finally arrests it and causes fatty degeneration. When iodine instead of iodoform is allowed to volatilise in the moist chamber the blood under examination coagulates too quickly and too generally. Carbolic acid and salicylic acid as well as iodoform, quinine, and eucalyptol have the power of arresting the formation of pus in the frog. Prudden found that irrigation with a dilute solution of carbolic acid (1 in 1,600) stops diapedesis almost entirely. When it is washed off with half per cent. salt-solution, the diapedesis again begins, and this may be repeated several times alternately. Solutions of salicylic acid (1 in 4,000) have a similar effect. These five substances alter the leucocytes without producing any visible effect on the vessels. Binz supposes the diapedesis to be due to the vital properties of leucocytes, and not as Cohnhein and Hering believe to an alteration in the wall of the vessel. Binz seems inclined to think that the leucocytes pass through the vascular wall by fastening themselves to it and digesting it at the point of attachment, so as to form a passage for themselves. Peptone is largely found in pus corpuscles; and a relation appears to exist between the amount of peptone and the liquefying powers of the pus. The leucocytes act as destructive ferments on that part of the vascular wall to which they adhere for any time. They are destroyed by the vapour of iodine, etc., like all other organised ferments, and thus they leave the vascular wall unchanged and cannot penetrate through it. The same thing appears to hold in man, if the statement be correct that iodine and iodide of potassium are successful remedies in croupous pneumonia (*Deutsche med. Woch.* 28, 52, 1882). This can only be due, he thinks, to paralysis of the protoplasmic masses which enter the alveoli by the iodine which they themselves liberate from iodide of potassium.

The action of iodoform on wounds and on the organism generally is due to the iodine which is liberated from it. This is shown by the identity of the toxic action of iodine, iodide of potassium, iodide of sodium, and iodoform. They differ only in strength and in rapidity of absorption. It is quite possible that in the organism, organic combinations are formed intermediate between iodoform and iodine, but when these reach cells sensitive to the action of iodine, this element acts upon them, causing first stimulation, then paralysis, and afterwards complete destruction. This is not only true for leucocytes but for the cells of the cerebrum, as is shown by the fact that iodine, bromine, and chlorine have all a narcotic action. (*Virchow's Archiv*, Vol. 89.)

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Department of Public Health.

PROGRAMME OF THE SECTION FOR COLONIAL MEDICINE AT THE INTERNATIONAL COLONIAL AND EXPORT TRADE EXHIBITION, AMSTERDAM.

May—1883—*October*.

[At the request of the president, Professor Stokvis, we insert the following programme. We trust it will not only be interesting to our readers, but may induce such of them as are resident in distant countries to contribute if possible to the Exhibition.]

At the invitation of the Dutch Association for the advancement of Medical Science, the Executive Committee of the Colonial Exhibition have resolved to add a Section for Colonial Medicine, to be opened simultaneously with the colonial exhibition. Whereas it is the object of the colonial exhibition to diffuse general knowledge of colonies and transoceanic possessions, the aim of this medical section is a more limited one, and of a strictly scientific character, viz. to promote our knowledge of the sanitary and medical conditions in the colonies.

It will be divided into Three Classes.

The First Class will embrace everything relative to public health in the colonies and transoceanic possessions.

It is generally admitted, that the welfare of colonies is closely connected with the health as well of the European population as of the natives, but the opinions as to the best way to attend to the health of the population, under the various conditions of climate, tropical diseases, customs, and habits, are still unsettled, principally from want of a sufficient knowledge of facts. A comparative exhibition, therefore, of all that is done in this direction, affording at the same time as much information as possible for an accurate knowledge of the state of health of the population, may be called one of great interest. It will serve, it is hoped, as a guide to the Governments in introducing sanitary improvements or in remedying existing defects in the various colonies.

The Second Class is of equally practical importance. It will be devoted to all data connected with the organization of the medical service in the colonies, in regard to which the several Colonial Governments follow different systems, and it is only by a careful study of them that a correct judgment can be formed as to the best system to be adopted.

The Third Class will have for its scope attendance on the sick and wounded,

their transport, and also their treatment by the natives. In all civilized countries this attendance is a subject of public care and interest; but in the colonies there are special circumstances to be taken into consideration, such as are connected with the nature of the tropical diseases, the habits of the population, and the difficulties in obtaining the necessary means for the prope treatment, transport, and attendance of the patients. It is not too much to say, that a collection, as complete as possible, of everything bearing upon this subject would be instructive in the highest degree. A comparison of what is achieved in the colonies under extraordinary circumstances, with what is done in the mother-countries, will undoubtedly lead to results beneficial both to science and humanity.

This class will also comprise everything connected with the mode of treatment of the sick and wounded by the natives themselves. Both the natives and the colonists are in this respect actuated by the same motives of humanity, but the means they use are different. Although it may be contended that the means employed by the natives are for the greater part of a primitive character, yet it should not be forgotten, that many medicaments and modes of curing applied in medical science have been discovered by these very natives, and their salutary effect acknowledged by popular experience long before they were used in scientific medicine. All objects relative to these modes of treatment by natives, even those of which the origin is not or little known, will be very welcome to the Committee.

The Committee is well aware, that these three classes do not comprise all that a programme of a Medical Exhibition relative to Colonies should contain in order to be in any way complete; nor does the Committee forget, that in the subdivisions of the three classes a great many things are omitted that are important in more than one respect. But in view of the limited time allowed before the opening of the Exhibition, the Committee has been compelled to confine itself to the three classes mentioned. As, however, the subject is one of a strictly scientific character, it is, of course, not intended to exclude whatever may not have been specially mentioned. On the contrary, every contribution which may serve to promote the object of the Medical Exhibition will be thankfully received.

The space allotted to the Medical Exhibition being a limited one, models, plans, drawings, &c. of the objects will be preferred to the objects themselves, if the latter should be too voluminous. This, however, does not apply to those subdivisions of classes for which objects *in naturâ* have been solicited.

The space required for the Medical Section is allotted free of any cost. Objects of a commercial or not strictly scientific character will not be admitted. Exceptions may be made in such cases where duplicates are already exhibited in one of the other Sections.

The objects intended for the Medical Section will be received up to the 1st April, 1883, and the packages should be marked: "Exhibition of Amsterdam. *Section: Colonial Medicine.*"

It is intended to hold an International Congress for Colonial Medicine during the Exhibition in September, 1883. The programme will be published hereafter.

Class I.

The Ministration of Public Health in the Colonies.

1. Sanitary Commissions, their organization and labours; regulations and reports.

2. Supply and testing of water for drinking purposes. Models of artesian wells. Aqueducts, reservoirs, ice machines. Distilling apparatus, filters, &c.

3. Regulations against the adulteration of food. Testing of foods. Adulterated foods. Rules and regulations regarding the sale of opium, haschish, alcoholic and other intoxicating drinks. Intoxicating drinks made and used by natives.

4. Bathing and sanitary establishments.

5. Schools, dwellings for the labouring classes, barracks, prisons, &c., from a hygienic point of view. Sanitary control.

6. Burial grounds and cremation. Sewerage, sewers, cesspools, tubs, &c.

7. Trades, professions, and cultures detrimental to health. Communications in regard to the diseases caused thereby; sanitary control; means of improvement.

8. Measures against endemic, epidemic, and contagious diseases. Measures against diseases caused by marshes, even when drained, by inundations, &c., in connection with the origin of malaria, bilious and yellow fever, cholera, dysentery, &c. Measures for the prevention of parasitical diseases, (The Committee particularly desires the contribution of objects relative to parasitical diseases proper to the colonies.) Rules and regulations on prostitution, and measures for preventing the spread of venereal diseases. Rules and regulations on vaccination. Establishments for animal vaccination. Models, plans, and drawings of establishments for quarantine. (Buildings, vessels, barges, &c.) Regulations for the prevention of epizootic diseases (rinderpest, &c.) Objects relative to these diseases.

9. Death-rates. Diagrams of mortality in colonies, if possible with indication of the sources whence the returns have been compiled, and of the method followed in grouping them.

Class II.

Organization of the Medical Service in the Colonies.

10. Direction and Administration of the Civil and Military Medical Service. Laws and regulations relative to the qualifications for the practice of medicine, surgery, midwifery, pharmacy, and veterinary surgery. Laws and regulations relative to the military medical service. Rules and regulations on the practice of medicine, pharmacy, and veterinary surgery by Chinese and other native practitioners. The distribution of physicians, surgeons, midwives, apothecaries, and veterinary surgeons in the colonies.

11. Special medical education of colonial doctors in the mother-country. Medical schools in the colonies (education of Doctors-Djawa, district-doctors, &c.), Regulations. Buildings. Text-books, and other auxiliary means of education (anatomical preparations of tropical and exotic diseases, &c.).—Schools for midwives. Regulations. Buildings. Text-books. Educational accessories. —The instruction of vaccinators. Regulations. Text-books.

Class III.

Attendance on, and Transport of, the Sick and Wounded; their Treatment by the Natives.

12. Models, plans, and drawings of civil and military hospitals, lying-in-hospitals, lunatic and leper asylums, ambulances in the colonies, &c. Objects used in nursing the sick and derived from the colonies.

13. Means of transport of sick and wounded. *By land:* preliminary assistance, ambulance-carriages, brancards. *By water:* hospital and transport-ships and boats.

14. Medical literature printed and published in the colonies (periodicals, pamphlets, treatises, and other medical publications).

15. The medical treatment and hygienic customs (circumcision, &c.) of the natives. Instruments, contrivances belonging thereto. The drugs of the natives, if possible with a correct description of their origin and application. Medical literature of the natives. Chinese apothecary's shop.

16. Means used by natives (antidotes and medical treatment) in cases of poisoning and poisoned wounds (bites of poisonous or injurious animals—as snakes, spiders, centipedes, scorpions, mosquitoes, &c. ; contact of toads, bats, &c. ; stings of some fishes; wounds from poisoned arrows, &c.). It is requested to send the native poisons themselves, with a correct description of their effect according to the appreciation of the natives, adding also the animals or insects, arrow-poisons, &c. Treatment of the chica (*pulex penetrans*) by the natives.

17. Obstetrical help and treatment of mother and child during and after the confinement, as practised by the natives. Instruments, &c.

The Committee of the Colonial Medical Exhibition are :—Prof. B. J. Stokvis, president ; Dr. A. A. G. Guye, vice-president ; Dr. F. J. van Leent, first secretary ; Julius Coronel, second secretary.

The Executive Committee of the Exhibition are :—D. Cordes, president ; S. de Clercq, delegated member ; J. Kappeyne van de Coppello, LL.D., secretary ; E. Agostini, Commissary-General.

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PREVENTION OF THE SPREAD OF CONTAGIOUS OR INFECTIOUS DISEASES.[1]

THE Provincial Board of Health of Ontario, Canada, has issued the following instructions concerning the measures to be adopted in preventing the spread of scarlet fever, diphtheria, small-pox, &c. :

These diseases are spread by means of infectious particles going from person to person, and can be stopped if care and

[1] *National Board of Health (U.S.) Bulletin*, Vol. III., No. 51.

systematic means are taken to destroy the disease germs, and to prevent their being carried from one person to another.

Those parts of the body which are the breeding-places of the contagious particles give off the poison in the greatest amount; for example:

In scarlet fever, the mouth, throat, nasal passages, and skin.

In diphtheria, the mouth, throat, and nasal passages.

In small-pox, the pustules, chiefly of the skin.

In measles, the skin and air-passages.

In whooping-cough, the air-passages.

In typhoid fever and cholera, the discharges from the bowels.

The portions of the body thrown off, and containing the contagion, may pass into the air, or find their way into water or food, and in this way be introduced by breathing, drinking, or eating, or through broken surfaces of the body.

It will be seen that the first five diseases in the above enumeration are very readily communicable through the atmosphere. The contagion of the first three remains virulent for a great length of time and at great distances, and they are also very serious in their effects. Our consideration will at present be chiefly directed to them in the following rules for checking their spread:

WHAT THE GENERAL PUBLIC MAY REASONABLY EXPECT FROM THOSE AFFLICTED WITH CONTAGIOUS DISEASES, THEIR FRIENDS AND THE MUNICIPAL AUTHORITIES.

1. When anybody, especially a child or young person, has sore throat, bad breath or fever, he should be kept separated from all other persons except necessary attendants, until it be ascertained whether he has scarlet fever, diphtheria, or some other communicable disease.

2. Every case of diphtheria, scarlet fever, or small-pox should be at once reported to the health officer appointed by the local board of health. (See sections 18, 19, and 20, Public Health Act of 1882.)

3. On receipt of such notices, the local health officers should

immediately verify the reports of cases. If medical attendant reports the case, this will be sufficient verification.

The board should secure isolation of those sick with, or exposed to, these diseases;

Give notice of infected places;

Regulate funerals of persons dead from infectious diseases;

Disinfect rooms, clothing, and premises;

Give certificates of recovery, and of freedom from liability to communicate the disease.

4. Every person known to be sick with scarlet fever, diphtheria, or small-pox should be promptly and effectually isolated from the public; no more persons than are necessary should have charge of the patient, and they should be restricted in their intercourse with other persons.

5. Notices should be placed on the house in which a case of scarlet fever, diphtheria, or small-pox exists, and no unnecessary persons allowed to enter.

CONDUCT OF SICK-ROOM AND ATTENDANTS.

6. The bedroom of a person sick with scarlet fever, diphtheria, small-pox, or any other infectious disease, should be cleared of all needless clothing, carpets, drapery, or any material liable to harbour the poison of the disease. The room should have a liberal supply of fresh air, at least 2,000 cubic feet per head per hour. In summer the supply should be unlimited; windows thrown open, and draughts on patient prevented by a fine gauze or wire netting, slanting from open point of sash to within two inches of ceiling.

7. Discharges from throat, nose, and mouth should be received, or immediately placed, in vessels containing some of the disinfectants named for that purpose in section 8; if on rags or handkerchiefs, they should be immediately burned.

Likewise, the discharges from kidneys and bowels should be passed into vessels containing a pint of disinfectant, and immediately buried at least a hundred feet from any well or other drinking-water supply. If these precautions are

impracticable, let the excreta be passed on old cloths, which should immediately be burned.

8. For convenience, a few disinfectants are here grouped together:

(1.) Solution of chlorinated soda (or lime).

(2.) Solution of chloride of zinc: water, 1 gallon; sulphate of zinc, 4 ounces; common salt, 2 ounces.

(3.) Solution of chloride of lead: dissolve two drachms of nitrate of lead in a quart of water; then, in a large vessel containing a gallon of water, dissolve two tablespoonfuls of common salt (chloride of sodium); mix the two solutions together and store for daily use.

The above will answer for all purposes of disinfection: discharges, linen, cups, &c.

(4.) Carbolic acid solution, say 1 part in 20 to 40 of water, for discharges and clothes, scrubbing floors, &c., and washing hands.

(5.) Copperas solution; 1½ pound commercial sulphate of iron to 1 gallon of water.

(6.) Carbolate of lime.

The two last for discharges; the copperas very good for privies; the carbolate of lime for sprinkling dry in rooms, sheds, &c.

The carbolic acid solution may be made to permeate the air by spray from an atomizer, and destroy germs in breathable air by actual contact.

9. *Purification of Clothes and Bedding.*—The best plan is by the agency of heat. Dr. Henry, of Manchester, disinfected scarlet-fever clothing by exposure to 212° Fahr. for one hour. An oven will answer the purpose, carefully removing the embers, and hanging on wires the clothes to be disinfected. Boiling clothes not so good as baking, but still useful. To every ten gallons of boiling water add half or three-quarters of a gallon of commercial solution of chloride of lime; or the clothes may be laid for twenty-four hours in a solution of sulphate of zinc in the proportion of 1 to 120, or of chloride of zinc in the proportion of 1 to 240, or in the chloride of lead solution (section 8), and then be washed with soap and water if they cannot be baked.

10. Nurses and attendants should be required to keep themselves and their patients as clean as possible, disinfecting their hands frequently by chlorinated soda or other disinfectant.

Attendants should also wear cotton or linen (not woollen) clothes or overalls, to which particles will not so readily adhere, and which may be more easily disinfected.

11. Every person recovering from diphtheria should be considered dangerous, and should not be permitted to associate with others, or attend any public assembly until the throat and sores on lips or nose are healed for some days; nor before, in the judgment of the physician, he can do so without endangering others, nor until all his clothing has been thoroughly disinfected. These restrictions, of course, extend to churches, schools, &c. Every apartment of the house must also be thoroughly disinfected before the patient is permitted to go at large.

After recovery from scarlet fever and small-pox a still longer time must elapse, to allow all particles of disease-bearing skin to be thrown off.

12. The body of a person who has died of diphtheria, scarlet fever, or small-pox, should be washed with a strong chloride of lead or zinc solution, double the strength of those in section 8, wrapped in a sheet wet with the same, and at once buried. In no case should the body be exposed to view; no public funeral held, and as few attend as possible.

DISINFECTION OF DWELLINGS AND PREMISES AFTER RECOVERY OR DEATH.

13. In addition to thorough cleansing of all wood-work with soft soap, and water to which carbolic acid has been added (one pint of the common liquid to four gallons of water), and to removing and washing all fabrics which can be removed in the manner described in section 9, and brushing the walls, the rooms should be fumigated for a period varying from three to twenty-four hours with sulphurous acid. All doors and windows and the chimney being tightly closed, and all fabrics to be washed taken away, sulphur is put into a metallic dish, a little saltpetre put on the top or mixed with it, and then lighted.

The proportions should be two pounds of sulphur for every 1,000 cubic feet of space. In a very long room it is best to have the sulphur in two or more places. After the fumigation is completed, the doors and windows should be opened, and kept open for several hours. In disinfecting in this manner with sulphurous acid, the person setting light to the saltpetre and sulphur must make a precipitate escape from the room the instant the sulphur is burning. Carpets may be fumigated on the floor by this method, afterwards removed to the open air and thoroughly beaten. Pillows, feather beds, mattresses, and upholstered furniture, after being disinfected on the outside, should be cut open and their contents exposed to the fumes of burning sulphur. In no case should the disinfection of clothing and bedding be omitted. Where articles of clothing, towels, or anything used by sick persons, are considered too valueless to be kept, they must not be burned in the house or open air before they have been completely disinfected. Bad epidemic at Philadelphia resulted from neglect of this precaution.

PRECAUTIONS FOR WELL PEOPLE TO AVOID SCARLET FEVER, DIPHTHERIA, AND SMALL-POX.

14. Avoid exposure to special contagion of the disease; more danger for children than for adults. Do not, therefore, let a child go near a case. Do not permit any person or thing, dog, cat, or other animal, plaything, letters, &c., to come direct from a case of these diseases to a child, unless previously disinfected under competent supervision. If you do visit a case, bathe yourself, especially hands, face, and hair, in a disinfectant solution, and change and disinfect your clothing before you go where there is a child. See that your residence, premises, &c., are kept clean and dry; that the sewer connections are well trapped and drains well ventilated. Never allow passages from persons sick with the disease to be placed in water-closets or privies but have them attended to as in sections 7 and 8. Give special attention to purity of milk supply. Do not allow a child to ride in any vehicle where there is suspicion of infected persons having travelled. Avoid exposure to wind, and cold

dry air. Do not wear or handle clothing worn by a person during sickness or convalescence from these diseases. Beware of any person who has a sore-throat or running at nose. Do not kiss or take the breath of such a person. Do not drink from the same cup or put pen in your mouth.

15. In the case of all these diseases, remember that the contagion may be stored up from one season to another if not destroyed. Do not let it be so stored, and see that your children do not visit a house where one of these diseases has been, even though some months have since elapsed, unless you know the house, clothes, &c., have been thoroughly disinfected.

16. In the case of small-pox too much care cannot be taken to see that every person who has not been vaccinated within seven years be vaccinated or re-vaccinated.

17. If vaccination has "taken well" a few years before, this is, if anything, an extra reason for re-vaccination. Persons who have had small-pox may take it again.

18. With regard to all these diseases, remember that a mild form in one person may originate a severe form in another.

19. In connection with this subject, it should be remembered that too much attention cannot be paid to surroundings in general, such as drainage, ventilation, food, warmth, &c. Temperature and rainfall have much to do with the spread of some of these diseases. Diphtheria for example, which is generally least prevalent in August, increasing until January, and with the same regularity declining again until August. Most fatal in lowest and worst-drained parts of cities. Examine relative positions of wells and privies. Where city water is used, investigate source of water supply, and place of debouchure of sewers (outbreak of diphtheria in Naples, 1872, clearly traced to contaminated water). In country districts, isolated outbreaks, traceable to cesspool effluvia, are not at all uncommon. Frequently will be found the water-closet drain discharging into a cesspool cleaned out only at rare intervals, gases generated in cesspool having no outlet except through water-closet and into the house; hence diphtheria and other diseases. In cities, where proper attention to the trapping of all waste-pipes leading to sewers is too frequently taken for granted, examine carefully into arrangement and ventilation of drain; ascertain

whether, in consequence of attention not having been duly paid to the trapping of overflow, lavatory, and every other waste pipe, gases are not being conveyed in sundry ingenious ways into the various apartments they were presumed to be excluded from; that the plumbers have not, in other words, succeeded in ventilating the house drains, and therefore, of course, the sewers, into the bedrooms.

THE PRACTITIONER.

FEBRUARY, 1883.

Original Communications.

THE REMEDIES IN THE TREATMENT OF EPILEPSY BEFORE THE INTRODUCTION OF THE BROMIDES.

BY JAMES RUSSELL, M.D., F.R.C.P.

Physician to the Birmingham General Hospital.

It fell to my lot to treat a considerable number of cases of epilepsy before anything was known of the influence of the bromides over that malady. The present communication states the result of the treatment employed; and it is perhaps significant of the limited power formerly enjoyed in controlling the disease, that out of a considerable number of cases (above 300) of which I have notes, there are but forty-nine in which the attendance has been sufficiently continued for my present purpose. Of these cases, however, the attendance was protracted sufficiently to afford an opportunity of testing the operation of more than one remedy. The inquiry as to the effect of the remedies employed is not without a practical end, for several of the medicines to be mentioned are still used to assist the action of the bromides. I cannot claim any very definite scientific purpose in selecting certain of the following agents; though sometimes the choice was determined by the circumstances of the patient. I should add that a large proportion of the patients were

out-patients, who are placed in the least favourable conditions for the favourable operation of remedies.

Iron.—Iron was administered to twenty-one patients variously, in the form of citrate, perchloride, iodide, and phosphate; it was employed as a general tonic, generally alone or with aperients, and in some cases with hypnotics at night when sleep was defective. In about one-half of the cases cod-liver oil was also prescribed. The period of administration was long, in a large proportion—thirty months; eighteen months; twelve months; in two cases ten and eight months; in eight cases for three or four months; and so down to thirty days. Manifest evidence of depressed physical health was not present in more than one-third of the patients. The following report, given as succinctly as is consistent with clearness, indicates the results.

(1) A girl; iron and oil for eighteen months; on some occasions reported "better," and "wonderfully better;" the fits fell to eight and four per month; but benefit was not permanent; and the case had a bad termination. (2) Iron with oil for twelve months; about thirty fits during the period; probably no important diminution of former rate. (3) Iron for eight months; fits reduced from weekly occurrence to two per month; but the case ended in the workhouse. (4) Fits only just appeared; iron for ten months; about twenty-three fits; but marked improvement mentally and physically. (5) Steel for five months; diminution in the daily fits; oil then added through six months longer; fits fell to one per month, increasing towards the end; strychnia added to the steel, the oil withdrawn through three months additional; increase in number and severity. (6) Iron with hemp, three months; then iron and strychnia through twenty-seven months; little improvement in the serious nervous depression, which was the prominent symptom, fits being few.

A better result was obtained in a few instances. (7) Lowered general health; iron given for three months; at once great diminution in number of fits, and final immunity for fifty-six days; health greatly mended. The fits returned with change of treatment. (8) Much anxiety and excitability; four months of steel; two fits only; health perceptibly mended, hemp

procured sleep. (9) Anæmia; iron with quinine, and then with zinc (2 grs.) for five months; fits lessened materially, only four in last two months; afterwards free for thirteen months. (10) Temporary diminution, but no lasting impression, in a girl whose fits occurred chiefly at the menstrual periods, from steel with oil through five months, and morphia at bed-time. Of the remaining cases in which the remedy was given, from four to one month, no amendment was procured in the epilepsy, but much improvement was effected in the general health. In none, with possibly a slight exception, was any increase noticed in the number of the fits.

Zinc.—It was tried in twenty cases, in most of them for long periods. Eighteen grains three times a day was the highest dose attained. The period of administration was through a year and a quarter in two patients; in others for sixteen, eleven, six, five, four months down to two months. In many patients the use of zinc followed failure of other remedies; hypnotics were called in to aid sleep when necessary. The ages of the patients varied from four to forty-seven years; five patients were under twelve years. In six cases a decided impression was made on the course of the disease, as follows:—(1) Given for eleven months, fits from frequent recurrence fell to one per week, with several intervals. (2) Through six months; daily fits fell to two or three per week with occasional freedom for a fortnight. (3) For nine months; fits suspended through first half with marked improvement on mental condition, but during the latter half mitior fits established themselves. (4) For three and a half months; fits entirely ceased, but reappeared at the end of the term. (5) Zinc after failure of other remedies; fits, having sometimes happened "with great frequency," fell to the rate of three to five per week. (6) Perfect freedom during four months of zinc, but fits had been infrequent previously.

The remaining cases exhibit only varying degrees of failure. After twenty-seven months only temporary diminution for four months, but no further impression; yet there was marked attestation of increase of mental control and energy, but the case ended in an asylum. For a year and a quarter, in another case, without improvement. But I must add that, in some of the

cases in which no control had been exercised by the zinc upon the number of fits, decided testimony was afforded to its beneficial action in improving general nutrition, and increasing mental vigour. In two cases especially the evidence in this direction was very strong, although the fits remained as numerous as before.

Arsenic.—It has been given in nine cases. In three from eleven to eight months. In three from two to four months. Eleven minims of Fowler's solution the highest dose. (1) It produced a decidedly beneficial result in a case of partial hemiplegia from infancy, with fits at the rate of five or six per day. Given for eleven months, the fits were reduced to two per week with immunity for three weeks, but returned on withdrawal of the medicine. (2) Given for eight months in a case in which the fits were frequent; the number was reduced to eight in the period of administration. (3) Arsenic with hemp, given for a "lengthened period," effected decided amendment in the mental condition of a patient exhibiting a considerable degree of mental infirmity. (4) The remedy in combination with opium (opium alone having failed) effected considerable diminution in clonic spasms, very prominent in an epileptic, the spasms increased when the medicine was omitted.

Strychnia.—I added strychnia to other remedies, chiefly to iron, on their failing to answer the purpose I had intended. I was guided in my selection of strychnia by various evidence of decided failure of nervous or mental power. The result, on the whole, has not been promising. In a subject with much nervous depression, but few fits, strychnia to $\frac{1}{12}$ gr. with steel and oil was continued through twenty-seven months; only eight fits occurred, though some slight ones also made their appearance; but the evidence of nervous deterioration increased, with marked depression, and the nights, which had improved, again became restless. The same dose, for four months, was given to a man of very sensitive and irritable nervous constitution, with feeble self-control. The fits rather increased in number, but the nervous degeneration did not advance; nevertheless the case ended in an asylum. Increase in the frequency of the fits followed the addition of $\frac{1}{16}$ gr. strychnia to steel which had been previously administered

without benefit. In other two cases addition of strychnia to remedies previously given afforded no advantage.

Opium.—Opium or morphia has been employed in a distinct form in fourteen patients, chiefly for certain nervous derangements specially incidental to epilepsy. The derangements had assumed the form of morbid irritability, and of feeble control over erratic nervous developments, manifested particularly in disturbed sleep, dreams, and nocturnal alarms. Such derangements have been distinctly associated, in the patients' history, with the setting in of the fits: in two instances their commencement took place under my own observation. One patient never wanted to go to sleep, he was calling out all night long. Another was so frightened at night that she would not leave the chamber where her brothers and sisters slept. Nocturnal starts kept one patient awake; he was wrapt in a blanket and taken out of bed. A woman used to awake her husband from her fright; another feared to go to sleep; a third "woke up" with palpitation. With these disorders hysterical and hypochondriacal manifestations were combined. Improvement in sleep was generally, but not universally, attended with diminution in the frequency and severity of the fits. In six of the cases more or less amendment in these nocturnal troubles was effected by the opium. (1) Morphia at bed-time, through nine months, effected very decided improvement in all the symptoms. (2) Small doses of laudanum administered during the day through four months in combination with steel, procured improved sleep and lessened the number of fits; but they resumed their original frequency at the close of the period. In three patients failure in the remedy to relieve the symptoms was more or less complete, even though opium was continued through five months in one patient; in one patient morphia utterly failed, and although there were no fits during the time, mental deterioration advanced steadily.

Five cases stand apart. (1) In one opium is stated to have been of special service in the delirium to which the patient was liable. (2) In a girl suffering from protracted tonic spasm of the rectus abdominis, daily hypodermic injection of morphia failed. (3) A boy hemiparetic, and suffering from daily fits from infancy, took laudanum up to ♏23 three times a day

through nine months; after the fifth month the fits gradually fell from the rate of four or five per day to two or three per week, and in each of the last two weeks there was but a single fit. (4) Laudanum given during the day seemed to control the "wildness" of a patient, but did not avert the necessity for sending him to an asylum. In the fifth case no advantage resulted from opium given by day.

Cannabis Indica.—It was administered to fourteen patients; to five as a simple hypnotic, in two of them with very good effect. In the other nine patients it was given by day with special reference to the nervous condition of the patient. The description of the cases selected is but a copy of that already given in the section on opium: nervous derangement, irritability, disturbed nights, and nocturnal alarms; in certain cases the presence of anxiety and worry appeared on the patients' history. The dose varied from fifteen to twenty-five minims three times in the day. The general result may be stated as entire absence of benefit in three cases; in the remaining, temporary improvement either of the mental condition or in the number of fits, or in both respects; but no permanent amendment; the period of administration extended from two to four months. It was remarked that the remedy seemed to exercise greater control over the occurrence of the fits than over the nocturnal disturbances.

Belladonna.—Belladonna in the form of tincture or extract was given alone only in six cases; in all of the six, but one, physiological effects are reported to have been produced. In three cases no benefit at all was effected by the remedy, though in one of the three it was continued through six months. In one case, severe tonic contraction of the rectus, which had resisted other measures, was speedily relieved by the belladonna. In one of the remainder there was diminution of the fits from two or three per day to three per week; but the diminution was not permanent; and in the remaining case the patient had only two fits in three months, but mental deterioration advanced.

In six other cases belladonna was combined with zinc; in one case only could any improvement be discovered; in the lengthening of the free intervals. In one case a prolonged trial of zinc alone gave evidence that no advantage had been gained

from combining it with belladonna, as had been done through eight months.

Atropine.—I employed atropine in six cases. The highest dose was $\frac{1}{12}$ gr. three times a day; marked physiological effects were produced in all the cases, even of a severe character. The period of administration varied from six months in one case; and three or four and a half months in two; to shorter periods, from forty-seven days to twenty-two and fifteen. The result was unfavourable. (1) Very decided diminution of fits during forty-seven days of administration, as compared with other remedies. But delirium and increase of the fits ended the period. (2) No fit during the three months; at the end mental excitement and delirium. (3) Given during six months, atropine effected temporary cessation of "very frequent" fits; but mania of a filthy character set in, and the fits finally increased to 100 in a week. (4) Severe fits were suspended during fifteen days, but then returned. (5) During four months, the fits rose from three or four per day, to twelve. (6) Twenty-one fits occurred during twenty days of atropine administration; twelve in a single day.

Phosphoric Acid: and Phosphorised Oil and Ether.—They were tried in three and two cases respectively, with no encouragement to continue the remedies.

Inhalation of Chloroform.—It was employed in four cases. It succeeded repeatedly in averting the fits in one case. The remedy failed in the other cases.

Ice to the Vertebral Column.—It was employed in five patients, all residing in the hospital. (1) Applied once and then twice a day for three hours through twenty-three days, the fits were reduced to one half. (2) Applied daily for three or four hours through thirty-five days, thirteen fits occurred, probably not a reduction. (3) Applied through a week daily "for a considerable length of time," it was thought rather to increase the number of fits. (4) In another case it failed entirely, but the trial was imperfect. (5) In the remaining case it was one of the remedies which failed in reducing the tonic spasm of the rectus abdominis already spoken of. I should add that in no case did we succeed in observing any physiological effect produced by the application of the spinal ice-bag.

Blisters to the Nape.—They were used in six cases. In one the patient affirmed he was relieved.

Seton.—A seton was introduced in one desperate case, suggested by the apparent effect of a burn in suspending the fits ; it remained for sixty-three days with some apparent benefit at the first. In another patient a seton remained for 119 days with brief improvement. In neither case was the relief lasting.

One patient referred the interval of nine months between his first and second fit to the introduction of a seton; it fell out at the end of nine months and two months after the second fit occurred.

Positive Electricity.—Charging with positive electricity from a static machine was tried without benefit in one patient for fourteen days.

MEDICAL ANNOTATIONS CONCERNING EPILEPSY, PARALYSIS, AND OTHER DISORDERS OF THE NERVOUS SYSTEM.

BY DR. RADCLIFFE.

(*Continued from* Vol. xxviii. p. 183.)

4. *On the Treatment of Epilepsy.*

I.

At a meeting of the Royal Medical and Chirurgical Society in May, 1857, after a paper on epilepsy by Dr. Sieveking, the President, Sir Charles Locock opened a discussion, which led to nothing else of importance, by giving an account of certain cases of epilepsy in females in which he had used bromide of potassium with very excellent results. Having told us that undue erotic excitability was apparently the cause of the attack in each of these cases, and that he was led to try this particular medicine by a short account, in the *Medico-Chirurgical Review,* of what some German physician who had tried it upon himself had observed regarding its remarkable anti-erotic power, he went on to say: "About five years ago I was applied to by the parents of a lady who had had hysterical epilepsy for nine years, and who had tried, without benefit, all the remedies which could be thought of by various medical men, myself among the number. This patient began to take the bromide of potassium at the close of a menstrual period in which she had had two attacks. She took ten grains three times a day for three months; then the same quantity for the fortnight preceding each menstrual period; and for the next three or four months only for a week before menstruation, but

during the whole of this time she had no attack. I have also tried the same remedy in fourteen or fifteen cases, with the most excellent results in every case except one, and in this one the patient had fits, not only at the time of menstruation, but also in the intervals." At the close of the meeting, in a conversation with Sir Charles in our walk homewards, I mentioned a piece of experience of my own as having possibly a bearing upon what he had said, namely this: that I had for several months been in the habit of giving *iodide of potassium*, along with bicarbonate of potash, in epilepsy, with decided advantage, especially in cases where the patient could not be persuaded to eat slowly and stint himself in food, and at the same time I expressed an opinion that possibly the bromide might do good by acting, to some extent, in the same way as that in which the iodide and bicarbonate had acted: that in the cases cited it might not be right to think that it did good only by calming sexual irritability. I also said that the beneficial action of the iodide and bicarbonate seemed to be neutralised by the addition of iron, and that the conclusion I had come to was, that iron was actually mischievous in cases of epilepsy. And to these remarks the reply was: "Um! about iron I am very much disposed to agree with you, about the other matter we must have another talk." This talk unfortunately never came to pass, but I have reason to know that my remarks upon the action of iodide of potassium and bicarbonate of potash on this occasion were not thrown away. On the day following this meeting and conversation, I saw a boy, æt. 17, who had been taking every day for three months, with unmistakable benefit, a dose containing three grains of iodide of potassium and fifteen grains of bicarbonate of potash. For five years before beginning to take these doses he had had on an average three fits a week; while under this treatment the fits had been reduced to one a week. I now added to the dose fifteen grains of bromide of potassium, and gave two doses in the course of the twenty-four hours, one at bed-time and the other on rising in the morning. I never saw the boy again, but his father told me that he had gone on steadily with the medicine for twelve months, that then he had an attack of fever which proved fatal, and that "the fits had been completely cured by the medicine." This

patient was, I have every reason to believe, all his life a pattern of morality and purity.

Two years later, in one of the Gulstonian Lectures which I had the honour of giving at the Royal College of Physicians in London, in February, 1861, I was able to say: "After trying it in scores of cases during the last two years, I can testify that the bromide of potassium is often a very valuable remedy in cases of epilepsy in which there is not the slightest sign of undue erotic excitability. I can testify, indeed, that this remedy has proved to be more or less serviceable in cases the most dissimilar in character, so serviceable that the name of Sir Charles Locock ought always to be remembered with gratitude by every epileptic, and by many suffering from other forms of convulsive disorder. How to explain the *modus operandi* of this medicine is no very easy matter, but I am inclined to think that this, in part at least, is by an alterative action upon the blood, analogous to that of iodide of potassium and common salt—an action by which, possibly, the blood may be freed from uric acid, and compounds more or less allied to it. And this I do, because for a long time I have found decided benefit from occasional doses of carbonate of potash, with two or three grains of iodide of potassium, or with a drop or two of tincture of colchicum, or wine of white hellebore." At this time I had also tried bromide of iron in place of bromide of potassium, with unsatisfactory results, for in the same lecture I say, "It appears to me pretty clear that bromide of iron is not to be preferred to bromide of potassium, for on looking over the notes of thirty cases in which these two medicines were tried for six or eight months, month by month alternately, I find that the beneficial effect of the preparation of potassium was very much the more marked of the two." These remarks were delivered before a full audience in the theatre of the Royal College of Physicians, on the 29th of February, 1861, and published first in the *Lancet*, on the 23rd of June in the same year, and a few months later in the third edition of my book (in which these Gulstonian Lectures were incorporated) entitled *Epileptic and other Convulsive Affections of the Nervous System, their Pathology and Treatment.*

Four years afterwards, speaking upon this subject in one of his

excellent lectures on the "Recent Advances of our Knowledge in the Diagnosis and Treatment of Functional Nervous Affections,"[1] Dr. Brown Séquard says: "This invaluable medicine (the bromide of potassium) has been admirably studied by my friend Dr. C. Huette, who showed[2] how different it is from the iodide of potassium. The principal features of distinction between these remedies are that the bromide is useless against syphilis, and that it induces sleep, produces amblyopia and deafness, diminishes the sensibility of the fauces, urethra, conjunctiva, and sometimes of the skin generally—that it does what the iodide does not do. We owe to Sir C. Locock the important discovery of the usefulness of the bromide in those cases of epilepsy where the affection is due to disturbances in the female genital organs. Dr. Radcliffe was one of the first who employed this medicine in all cases of epilepsy, but, following the example of Sir C. Locock, he, as well as several other physicians, gave a dose which was not large enough. My able colleague, Dr. Ramskill, and myself, after having tried the bromide of potassium in doses of from ten to twelve grains three times a day, as recommended by Sir C. Locock, found that these doses ought to be raised to twenty grains or more three times a day, and that the beneficial effect of these doses soon became quite manifest. Much larger doses have recently been employed in France, but I doubt if more benefit has been obtained, while, on the other hand, I hear that more than one case of death has taken place which seems to have been caused by the bromide. I can state that I have never seen any worse effect from a prolonged use of this remedy—for one or two years, with hardly any interruption, at an average dose of three scruples a day—than sleepiness and deafness, in great measure due to the nervous complaint, and to a feeling of fatigue." Dr. Brown-Séquard also allows that the usefulness of the bromide in epilepsy is notably increased by the addition of three or four grains of iodide of potassium to each dose.

In his *Lectures on Diseases of the Nervous System*, published in 1878, after quoting the remarks of Sir Charles Locock which I have already quoted, and saying that he began to use bromide

[1] *Lancet*, March 10, 1866.

[1] *Mémoires de la Société de Biologie*, vol. ii. p. 19, 1850.

of potassium in 1860, Dr. Wilks goes on to say: "It was in the early part of 1860 that I began to use the bromide of potassium in the treatment of epilepsy: in the following year about a dozen cases were published in the *Medical Times and Gazette*, being the first series of cases systematically described which I can find, in which this remedy had been found eminently successful." I do not know whether Dr. Wilks, if he had sought more carefully, would have found any other evidence to interfere with the claim of priority in this matter which is here implied. Soon after this time abundant evidence is forthcoming as to the beneficial action of bromide of potassium and ammonium in cases of epilepsy, and certainly long before this time, as I have pointed out, I had already, as I believe, won the race for priority. At all events, in one way or another, soon after this time, it came to be admitted on all hands that one or other of the bromides was the remedy of remedies in epilepsy; and this, with little or no care as to the question of priority, is the point upon which I wish to insist at present.

I am generally very ready to bow to the opinion of Dr. Brown-Séquard, but I cannot allow that he is right in saying that at first the doses of bromide of potassium which I was in the habit of using were "not large enough." At first I gave fifteen grains of this preparation twice a day, either by itself or along with bicarbonate of potash and iodide of potassium. Shortly afterwards —certainly a full twelvemonth before the date of Dr. Brown-Séquard's published remarks on the subject—I very generally gave three doses of fifteen grains in the course of the day, or rather one dose of thirty grains and a second of fifteen grains; and ever since this has been my practice. Now and then I have gone as far as ʒj. in the course of the day, for a short time, but never further, and certainly I have never been tempted to use larger doses by what I have seen—and I have had full opportunity for exercising my judgment in this matter —of the practices of those who have been in the habit of using these larger doses. What I have always found is that the bromide does not act kindly in cases where the memory is bad and the mental power generally enfeebled. The mischief done, as a rule, showing itself chiefly in stultification and in disfigurement of the skin, by rashes of various sorts, without any

very certain change for the better on the attacks. I have indeed found that the attacks were less likely to be kept in check if the bromide was pushed to the extent of causing any stultification or much cutaneous disfigurement, and that it was never advisable to go so far as to produce "bromidism," which, to my mind, is an evil which is scarcely less ghastly than epilepsy itself. I am quite satisfied that harm rather than good is done by giving large doses of bromide of potassium or bromide of ammonia in ordinary cases of epilepsy where the memory is bad and the mental power generally enfeebled, and that forty-five grains in the course of the day is too large a dose; rather give too small a dose for an adult in such a case. In a word, the conclusion at which I have arrived is that in any case the bromide has been pushed too far if it gives rise to any marked symptoms of "bromidism," that in cases of *le haut mal* with much mental enfeeblement this medicine is very likely to be hurtful even when only given in moderate doses, and that in the majority of cases of *le petit mal* the good to be done by it is barely appreciable.

Bromide of potassium, bromide of ammonia, and bromide of sodium—the three bromides most commonly in use, singly or together—appear to be very much on a par as to remedial value. I am constantly ringing changes upon the three, without any good reason it may be for doing so. On the whole I am most disposed to use the bromide of ammonium as the least likely of the three to stultify and to disfigure the skin, and also as containing more bromine, for on looking at the equivalents of these three bromides it is found that the quantity of bromine is eighty to eighteen in bromide of ammonium, eighty to thirty-nine in bromide of potassium, and eighty to twenty-three in bromide of sodium.

Of the bromide of iron—which is the only other bromide of which I have any practical knowledge—I have already said what I have to say on the subject. Since the delivery of the Gulstonian Lectures, in which I spoke unfavourably of the action of this medicine, I have never given it a trial, but the matter does not exactly remain *in statu quo*, for in the interval I have often had abundant proof that the good to be got out of the bromides of potassium, or ammonium, or sodium is not unfrequently

counteracted by the addition of any proportion of iron. In fact, I do not know where to turn in order to find any reason for believing that iron may ever be advantageously associated with the ordinary bromides in the treatment of epilepsy. On the contrary, I am continually meeting with some patient who is manifestly benefited by leaving off the iron which he has been taking along with bromide of potassium, or ammonium, or sodium, and who, without the iron, requires less bromide to control the attacks to the same degree. Upon this point I have no manner of doubt.

About the advantages of associating iodide of potassium and bicarbonate of potash with the ordinary bromides in the treatment of epilepsy I cannot speak so positively as about the disadvantages of associating iron with them. So far as I can judge from what I have seen in practice the beneficial action of the ordinary bromides is enhanced by the addition of iodide of potassium and bicarbonate of potash in cases where the memory is good and the appetite for food a little too good. In other words, I am inclined to think that less bromide will serve to produce a good result with this addition than without it. To this conclusion I am constantly led by the results of experience. But I cannot think, as I was once inclined to do, that the iodide of potassium and bicarbonate of potash are more than adjuvants—that they are entitled to take the place of the bromides in any case.

Among the other medicines which may be advantageously associated with the ordinary bromides in the treatment of epilepsy, those which seem to me to be most deserving of honourable mention are arsenic, hypophosphite of soda, and chloride of ammonium.

All along I have been in the habit of using arsenic in many cases of epilepsy, partly with a view to prevent or cure the skin disorder which is so easily produced by the use of the bromides, and partly as an anti-periodic tonic of the highest order of merit. I have an impression, amounting almost to a conviction, that the epileptic is often benefited by arsenic in very much the same way as that in which the aguish and neuralgic patient is usually benefited. At all events, I am quite sure that many epileptics, who cannot get on without the

help of the bromides and who do not take kindly to these medicines, take more kindly to them, and can do with less, when arsenic is judiciously administered along with the bromides. Nay, I may safely go farther than this, and say that there are many cases of epilepsy, among which are very many cases of *le petit mal,* in which the treatment which seems to answer best is a course of arsenic in very small doses with little or no bromide. I cannot say that the action of the arsenic is always beneficial, but I can say without hesitation that it is never hurtful, as iron seems to be always, and that it is generally beneficial if proper care be taken to give it in very small doses, say two or three minims of Fowler's solution for an adult, to suspend it whenever the stomach and bowels are out of order, and never to push it too far.

I scarcely know when I began to use hypophosphite of soda as a medicine in the treatment of epilepsy. From what I said at the time, it appears that I had been giving phosphorus dissolved in oil or ether for some time before the delivery of my Gulstonian Lectures in 1861; and, if my memory does not mislead me, it must have been shortly after this time that I gave up these unpalatable medicines and took to giving the hypophosphite. I looked upon the hypophosphite as a proper substitute for the medicines containing free phosphorus, partly because, like free phosphorus, it took fire at the approach of a lighted match, and partly because the breath smelt strongly of phosphorus very soon after a dose had been taken. I looked upon the phosphorus in the hypophosphite as all but free phosphorus, and at first I gave the hypophosphite for the sake of the phosphorus. Shortly afterwards I also gave it for the sake of the soda contained in it. By the phosphorus I hoped to invigorate nerve-power by supplying an important ingredient in nerve-tissue; by the alkali I hoped to work a change in the blood analogous to that which is brought about by the administration of bicarbonate of potash with iodide of potassium, and I have yet to learn that I was wrong in reasoning in this way and in acting accordingly. At all events, be this as it may, I have for years been in the habit of giving hypophosphite of soda in many cases of epilepsy, sometimes along with the bromide, and sometimes by itself, and generally with what

seem to me to be very satisfactory results. Indeed, I do not hesitate to say that the bromide often seems to be almost doubled in remedial value when it is given along with the hypophosphite, or that often thirty grains of the bromide along with thirty grains of hypophosphite, given in one or two doses in the course of the twenty-four hours, will go as far in controlling the attacks as forty-five grains of the bromide given by itself. And this is no small gain, for by diminishing the dose of the bromide the risk of stultifying and disfiguring the patient is to that degree diminished.

In this place also a claim for honourable mention may, I think, be safely made for chloride of ammonium. I have long been in the habit of giving this preparation along with the bromide as a substitute for bicarbonate of potash and iodide of potassium to the extent of from fifteen to twenty grains in the course of the twenty-four hours, and the conclusion I have come to is that it acts in very much the same way as the bicarbonate and iodide, and is a little less likely to overtax the vital energy of the patient. Only this very day, for example, an epileptic who had been taking bromide of ammonium with tolerable regularity during the last five years, sometimes by itself, sometimes along with bicarbonate of potash and iodide of potassium, and sometimes along with chloride of ammonium, told me that he always "got on best" when he was taking the bromide in the latter combination. And this patient is only one of many who have come to the same conclusion.

It is, I think, a mistake to be too ready to associate tonics and restoratives with the bromides in the treatment of epilepsy. When a tonic is wanted, as will happen now and then, quinine or bark or nux vomica, the latter more especially, may, I think, be tried with advantage for a time. When a restorative is wanted, as will also happen occasionally, the choice, I also think, may properly fall upon more than one. Sal volatile or sesqui-carbonate of ammonia is often used, and so is Hoffmann's anodyne, and with excellent results; but the restorative which pleases me most is a dessertspoonful of brandy or rum or whisky in the dose of medicine, or else a capsule containing a drop of œnanthic ether after it. I prefer this capsule where the question of cost need not be considered,

for I do not wish to encourage the habit of depending upon alcohol. I also prefer it because the restorative action of this ether is far more persistent than that of alcohol or any other ether. I have also given now and then, for the sake of the large quantity of œnanthic ether contained in it, a sip of Tokay in place of this capsule.

In the course of my professional life I have seen many cases of epilepsy in which a fair trial has been given to zinc, copper, or silver, to turpentine, naphtha, or camphor, to quinine or strychnia or nux vomica, in heroic doses, to valerian or wormwood, to stramonium, or henbane, to foxglove, to hemlock, to opium, morphia or cannabis indica, to cotyledon umbilicus or selinum palustre, to indigo, to poudre de Neuchâtel, to chloroform, to nitrite of amyl, to tracheotomy, to trephining the skull, to excising cicatrices, to cauterising the larynx, and other parts, to blisters, and setons, and issues, and to various other medicines and measures, and on reviewing my experience in these matters I fail to find anything anywhere which is calculated to enlist my confidence. I could mention not a few cases in which a strong faith in some particular medicine or measure or man has worked wonders. I could tell, for example, of an epileptic young woman, well known to me in my schoolboy days, who had had frequent fits all her life, and who was cured by wearing over her heart for two years a charm in the form of a small black silk package, which was an important source of income to an old woman living in the same village, and which, after the death of its proprietor, was found to contain nothing more wonderful than an old prescription by Mr. Abernethy. And I strongly suspect that without faith, much faith, the good to be got out of any of these medicines or measures, with the exception perhaps of occasional blisters or cauterisation, is not worth mentioning. At all events, I do not hesitate to say that there is no one among them which is at all comparable in remedial value to the course of treatment about which I have been speaking.

In dealing with the fit itself, it is, I think, a great mistake to raise the head. The head is raised on the supposition that the fit is caused by a rush of blood to the head, and this supposition is in no way defensible. During the fit the supply of red blood

to the head is cut off by an arrest of the breathing; and on theoretical grounds, therefore, what ought to be done is to keep the head down rather than to raise it up. And that this is the right practice I am quite satisfied, for again and again I have cut short a succession of fits in an epileptic by taking away a pillow, or by letting the head fall over the edge of the bed, or by raising the arms and legs so as to divert the course of the blood from them towards the brain. I do not say that no good is to be done by compressing the carotids, but this I say,—that if good is done by acting in this way, it is only by lessening the supply of *black* blood to the brain and not by lessening the supply of red blood. Nor, so far as I can see, is anything to be said in favour of the practice of fighting against the convulsions by force, or by sousing the patient with cold water, or by excoriating his nose and lips with smelling-salts. Indeed, all that need be done, so far as I can see, is to unloose the neckerchief and shirt collar, to lower the head, to prevent the patient from injuring himself by gentle means, and to wait patiently until the fit is brought to an end by the cessation of the process of suffocation, which cessation happens as a rule in a minute and a half or a couple of minutes, and is not to be expedited by any course of procedure with which I am acquainted.

II.

All is not done which can be done for the epileptic when a prescription for a medicine has been written out for him. On the contrary, there are directions about eating, and drinking, and working, and sleeping, which are all important, and about which much more might be said than I am at liberty to say here.

I am continually meeting with epileptics who are too ready to regard all kind of food, except animal food, as mere padding, who avoid fat for the fear of making themselves bilious, and who are benefited unmistakably as soon as they can be made to see that these are two grave blunders, and to act accordingly. The waste of tissue in vital action which has to be repaired by nitrogenous food, animal or vegetable, is not to be measured by the quantity of urea in the urine. This quantity is proportionate, not to the amount of work done, but to the

amount of nitrogenous food eaten. There is, in fact, reason to believe that a very large part of the nitrogenous food eaten finds its way into the urine as urea without having been used for nutritive purposes in its passage through the system, and that this way, to the very great prejudice of the eater, is often very much obstructed. In the liver the nitrogenous material set free by the waste of the tissues in vital action, and any surplus of nitrogenous material supplied in the food, is resolved into urea, which is eliminated by the kidney as excrementitious matter, and into an amylaceous substance called glycogen, which is burnt in the system as respiratory fuel. A decomposition is needed which cannot be properly carried out unless the liver be, so to speak, up to its work. An elimination is needed which is sure to miscarry unless the kidney be equal to a good deal of overwork. There is, indeed, a double difficulty to be dealt with, a difficulty attaching to imperfect resolution of nitrogenous material into urea and glycogen, and a difficulty connected with the imperfect elimination of urea. This is evident. And therefore it is easy to see that an epileptic or any one who is continually overtaxing his liver and kidneys by indulging a taste for meat, and other forms of nitrogenous food, is likely to be in better case when he turns over a new leaf in this respect.

It is also easy to see that an epileptic may blunder gravely who abstains from fat or butter, or oily matter, for fear of making himself "bilious." Olive oil does not disagree with the Italian or Spaniard, or ghee with the Hindoo, or blubber with the Laplander. These articles are taken freely in summer as well as in winter weather without giving rise to biliousness. They are taken as a matter of course; they are deemed to be so many necessaries of life; and no wonder. The amount of waste in vital action is to be measured not by the amount of urea eliminated in the urine, but by the amount of carbonic acid given out in the process of respiration. What seems to be wanted to repair the waste consequent upon vital action, is some carbonaceous article in the food, the burning of which may give rise to carbonic acid, and hence a reason for not excluding fatty and oily matter from the food. And further, this kind of matter may be wanted especially for the repair of nerve-tissues wasted in vital action, for nerve-tissue consists largely of a

particular kind of fat. In point of fact, it is easy to see not only that carbonaceous material is wanted to facilitate vital action, and that fat, or butter, or oil may supply this want, but also that these articles of food may be more suitable as fuel than the glycogen resulting in the decomposition of nitrogenous material within the liver. This glycogen is not forthcoming in proper quantity if the liver be in a sluggish or diseased state. The fatty and oily matter supplied in the food, on the contrary, is ready for use as soon as it is emulsified by the action of the pancreatic fluid and bile, and absorbed. They find some work for the bile to do outside the liver; they do not call upon the liver to do any work within itself like that which is done when nitrogenous material is resolved into urea and glycogen. Moreover, the creamlike emulsion which is formed by the admixture of fatty and oily matters with pancreatic juice and bile finds its way into the circulation without going the round of the liver. I can understand that a person may be "bilious" who abstains from fatty and oily matter, because the bile which is poured into the bowel has no fatty or oily matter to work upon; I cannot understand the association of biliousness with the contrary state of things, provided only the matter in question be taken in proper quantity. At all events, be this as it may, my experience in the treatment of epilepsy teaches me that an epileptic is much more likely to get on satisfactorily who is not afraid to take a liberal allowance of fat, or butter, or cream, and who makes a point of stinting himself in nitrogenous articles of food—in butcher's meat more especially.

As it seems to me it is also a grave mistake to look upon milk as a simple drink which may be taken with advantage at any time in almost any quantity, and which is almost always to be preferred to any drink containing alcohol. Instead of being a simple drink which may be taken like water, milk is food rather than drink; two tumblerfuls, or thereabouts, being equivalent in nutritive value to a mutton cutlet. To allow milk *ad libitum* to a person who in all probability is already overfed, is a practice which cannot be defended on rational grounds. It is a practice which calls upon an overtaxed stomach to pay a heavier tax, and this, in the case of an epileptic, is what ought not to be done, for without doubt an

overloaded stomach is a very frequent cause of the attack. In such a case, what is wanted at a meal in the form of drink is likely to be water, or some very light wine with water in small quantity. Indeed it is manifest that milk must be given very sparingly in such a case unless it be intended to put the patient upon a " milk diet." I am ready enough to allow milk almost *ad libitum* in this case, for upon no point in the treatment of epilepsy am I more satisfied than this, that an epileptic fared better in the days when "milk diet" was in vogue than he does in these days when it is too much the fashion to look upon any food except meat as little better than mere padding. What I am saying here, however, is intended to apply not to buttermilk or whey—especially when sour or sourish—but to fresh milk. In these drinks, as is abundantly exemplified in the history of people who are in the habit of taking them, there is something which at one and the same time promotes the digestion and warms the system to no inconsiderable degree, and it is not altogether unintelligible that it should be so. Lactic acid, upon which the sourness of these drinks is dependent, may very well promote digestion because it is an important ingredient in gastric juice. And it is quite conceivable that lactic acid may warm the system by supplying respiratory fuel. It is quite conceivable, indeed, that the saccharine articles of food may act as respiratory fuel, not directly, but indirectly, that is, by being first transformed into lactic acid, for the chemical composition of one atom of grape sugar is equivalent to two atoms of lactic acid. Moreover, it is difficult to explain the fact that the blood is not acid from the presence of lactic acid, except upon the supposition that the lactic acid is oxidized as soon as it finds its way into the vessels; that, in fact, it plays the part of a very combustible respiratory fuel—of the most combustible of these fuels it may be. I throw out these hints in passing without insisting upon them. Indeed, all that I wish to do here is to recommend a trial of some buttermilk or sour whey, sweetened or not according to the palate of the patient, as a drink in cases where a tumbler of fresh milk, or a jorum of rum and milk, or brandy and milk, or whisky and milk, is now often given. Be the explanation what it may, this trial has led me to a conviction which is altogether in favour of

this recommendation. Indeed, so satisfied am I with the beneficial results of this trial, that I am very much disposed to think that a time will come in which it will be held that the dietetic value of the products of the lactic acid fermentation is scarcely, if at all, inferior to that of the products of the vinous fermentation.

I do not agree with those who hold that in epilepsy water is always a more desirable beverage than any form of alcoholic drink. I am opposed to the use of any strong form of alcoholic drink, but not to the use of light table beer or a light wine like Petit Bordeaux. In saying this, however, I am not exactly pleading in favour of alcohol. There is very little alcohol in Petit Bordeaux, which, with or without water, is the best of all drinks, or in small table beer. There is also a flavouring principle, which gives the bouquet in wine, œnanthic ether. And, all things considered, I am disposed to think that the drink owes its virtue quite as much to this flavouring principle as to the alcohol. Œnanthic ether acts like ordinary ether, but owing to its remarkable want of volatility—its boiling point is not lower than 350° Fahrenheit—its action is much less sudden and far more persistent than ordinary ether. And what may be said of œnanthic ether applies also, to a certain extent, to the flavouring principle in beer. In the manufacture of beer, I am disposed to think that the change was for the worse rather than for the better when hops were made to take the place of wormwood, for the bouquet, so to speak, of hops is far less potent and persistent in its action than that of wormwood. I am also disposed to think that a very important fact has been overlooked in the manufacture of wines, and that drinks, owing their virtue to œnanthic ether, and not to alcohol, may be made from the refuse of the wine-press, which are scarcely inferior in real value to those made from the juice of the grape. And I venture to suggest to those who have to do with the manufacture of wine and œnanthic ether that they would do well to themselves and others if they would turn their thoughts to the making of wine rich in bouquet rather than to the making of wine rich in alcohol, and, after having done this, try to supply us with drinks in which water is flavoured with the œnanthic ether which is now, with great difficulty, prepared for the purpose of making British brandy and other vile and

execrable compounds. They would do well to themselves, for, in comparison with the wine, the products of the waste of the wine-press would in all probability prove as valuable as, in comparison with the gas and coke, the products of the waste of the gasworks has proved to be. And they would do no small amount of good to others, if I am right in the estimate I have formed of the dietetic value of œnanthic ether. And this is all that I feel called upon to say now upon the subject of beverages, except this—that, in very many epileptics, coffee or cocoa or chocolate appears to be preferable to tea.

I also hold it to be a mistake—the greatest of all mistakes—to suppose that in epilepsy the mind ought to be allowed to lie fallow, and the muscles put in action in every possible way. I have always thought and said that education and regular occupation of one sort or another were the true basis of any successful plan of treatment, and all I have seen from the beginning of my professional life to the present day has only served to convince me that I was not wrong in so thinking and saying. By all means let all be done that can be done to improve the general health by exercises of various sorts—by gymnastic exercises, which expand the chest, more especially—but never let it be supposed that education and occupation are not of paramount importance. Don't "put the cart before the horse." Indeed, so strongly do I feel upon this point, that I never care to be responsible for the welfare of an epileptic whose mind, in spite of all I can say, is allowed to lie fallow.

And, lastly, I think it very probable that the epileptic is often prejudiced by being allowed to indulge too much in sleep. The most frequent time for the attack is in bed or in getting up after a long night spent in sleep. And why? Is it that too much sleep has helped to bring about the fit by stultifying the patient? Too much sleep has a stultifying effect, and it is the stupid epileptic who is most likely to have frequent attacks: upon these points all are agreed. Whether what may be said of the stupid epileptic may also be said of the sleepy epileptic remains to be seen. I think it may: at all events, I am sure of this—that attacks which happen during sleep, or shortly after getting up in the morning, are often put a stop to or diminished in number and severity by shortening the hours spent in sleep.

THE TREATMENT OF EPILEPSY.[1]

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SUCCESS in the treatment of epilepsy depends first of all on accuracy in diagnosis. The maladies likely to be confounded with true epilepsy are :—

1. *In young children.* Convulsions from digestive disturbance, teething (?), worms, the exanthemata, tubercular meningitis, &c.

2. *In boys and girls and young adults of both sexes.* Hysteria.

3. *In adult women.* Hysteria.

4. *In adults of both sexes, but more usually in males.* Uræmia and convulsions from alcoholic or lead poisoning.

5. *At any period of life.* Convulsions may occur as the result of morbid growths in the brain, and where these do not reveal themselves by the ordinary signs of cerebral tumour (headache, vomiting, double optic neuritis) a correct diagnosis may be impossible.

In very young children I think we should be cautious in diagnosing true epilepsy, but I am deeply impressed with the importance of the view expressed by Sir William Jenner, that convulsions in young children are frequently a cause of chronic epilepsy, by setting up an epileptic habit in the nervous system.

Hysteria is a very frequent source of difficulty in diagnosis, especially when we have no opportunity of observing the fits. We may be aided by the absence of tongue-biting or injury from falling, and by the presence in the patient of hemianalgesia

[1] Read before the Midland Medical Society, October 25, 1882.

and amblyopia. In the hysterical fit, opisthotonos is, according to Charcot, always very marked and characteristic. Such fits are also usually of longer duration, and followed by outbursts of laughing or crying. It is, too, in hysterical fits that the patient struggles violently with those who endeavour to hold her, using often her teeth and nails upon the bystanders.

Uræmic fits can only be accurately diagnosed by discovering the presence of chronic Bright's disease (albumen and casts in the urine, high-tension pulse, cardiac hypertrophy, retinal hæmorrhages, &c.). The presence of albuminuria is not uncommon in true epilepsy, probably from the anæmic and dyspeptic condition of these patients. Generally speaking, it is necessary to exclude uræmia in all cases of epilepsy showing itself for the first time in an adult.

Alcohol and lead, and in France absinthe, sometimes give rise to epileptiform attacks, but a knowledge of these facts is sufficient to enable us to exclude this source of error.

With these brief remarks on diagnosis, I must pass on to the subject of treatment.

It is universally admitted at the present time that the bromide salts are by far the most powerful and efficient means we possess of arresting the convulsions of epilepsy.

Dr. A. Hughes Bennett[1] gives, as the result of his statistical inquiries, that the bromides checked the fits in 12·1 per cent., diminished them in 83·3 per cent., while in 2·3 the treatment had no apparent effect, and in 2·3 the number of attacks was augmented during the period of treatment. That is, in 95 per cent. of the cases these drugs proved themselves of value.

Pharmacologists teach that the bromides diminish reflex action, and stimulate the vaso-motor nerves, but in our ignorance of the pathology of epilepsy it is useless to speculate on the mode in which they arrest the fits.

The salts in common use are the bromides of potassium, sodium, and ammonium.

Bromide of camphor is very insoluble, and therefore difficult to administer. I have thought it useful in hysteria.

Bromide of lithium is recommended by Dr. Weir Mitchell, but it is expensive, like all the lithia salts.

[1] *Edin. Med. Journal*, 1881, pp. 706 and 704.

Bromide of potassium is undoubtedly the most popular in this country.

In America the sodium salt is said to be preferred, especially among the New York neurologists. Brown-Séquard recommends the administration of all three in combination.

The drug may be administered either in one large dose at bed-time, with a tonic during the day, or the dose may be taken in divided portions two or three times in the day. I prefer the latter plan for the following reasons:—

1. The same quantity of a drug generally acts more powerfully when given in divided portions than all at once.

2. The influence of the drug is kept up throughout the day.

3. Large single doses are more likely to cause depression and bromism.

As our object should be to control the fits with a minimum dose, I am in the habit of beginning with ten grains of bromide of potassium three times a day. In many cases this is sufficient. I invariably add to it ten minims of tincture of digitalis to counteract any depressing tendency.

At the same time I order some laxative to be used occasionally, and enjoin attention to the state of the bowels, as constipation acts frequently as a predisposing cause of epileptic attacks, even when the patient is under the influence of the bromide.

In general I recommend abstinence from alcohol, and so long as the patient's appetite is good and the general state of nutrition is fair, alcohol is more likely to do harm than good.

The diet should be rather meagre than liberal, especially in animal food; and the tendency to over-eat themselves, often displayed by epileptics, should be carefully checked.

If the fits do not cease under this treatment, I raise the dose of bromide, first by another ten grains of bromide of potassium, then by ten of sodium, and finally by ten of ammonium.

The most useful adjunct to the bromides is oxide of zinc. A pill containing three to five grains of this drug combined with one-sixth of a grain of extract of Indian hemp should be tried with each dose of the mixture when the bromides seem to be failing.

I have sometimes substituted tincture of belladonna for tincture of digitalis in obstinate cases, with, as I have thought, benefit.

As a rule, with only a small number of exceptions, this treatment is satisfactory; for even where it does not do all we wish, the patient is worse when he leaves it off.

In those unfortunate cases in which the bromides seem powerless, Dr. Gowers recommends borax in scruple doses, combined with one or two minims of liquor arsenicalis. Dr. Stewart Lockie,[1] of Carlisle, has reported a case treated successfully with this remedy after bromides had failed.

Dr. W. Law,[2] of Hastings, has recommended sodium nitrite, and Dr. Ralfe[3] has reported in favour of this drug; five cases which had not benefited from the bromides having improved under its use.

In the following cases, all of them rebellious to ordinary treatment, I give the results of my use of these remedies.

Case I.—D. F., aged 27, had been taking bromides under my care for three years; at one time he went for five months without a fit; latterly the fits have returned in spite of the medicine, and during the last week he has had three fits. He was (September 27th, 1881) ordered—

℞. Sodæ bibor. gr. xv.
Aquæ, ℥j.
t. d. s.

In the following fortnight he had three fits, and the dose was increased to a scruple. In the next eleven weeks he had nine fits. He was then ordered a pill containing five grains of oxide of zinc with each dose of the medicine.

In the following four weeks he had five fits, when cannabis indica, in doses of one-sixth of a grain, was substituted for the zinc, and afterwards increased to one-fourth of a grain. He took these medicines for nine weeks, during which time he had thirteen fits. I then ordered him a scruple of bromide of potassium, and ten minims of tincture of digitalis, which he has continued to take, having occasional fits, and every now and then a bad

[1] *Brit. Med. Journal*, October 21, 1882.
[2] *The Practitioner*, August, 1882.
[3] *Royal Med. and Chir. Soc.*, November 28, 1882.

outburst, but on the whole his condition has lately been favourable as to fits.

CASE II.—R. G., aged 21, has been under my care for eighteen months, taking bromides with little benefit; he has never been long without a fit, and has lately had several every week.

October 4th, 1881. Had had five fits in the last fortnight ordered—

℞. Sodæ bibor. gr. xv.
Liq. arsenicalis, ♏ii.
Aquæ, ℥j.
t. d. s.

In the following week he had seven fits, and the borax was increased to a scruple. The next week he had four fits, and after that one about every day for a week, when he was ordered—

℞. Sodæ bromidi, ℈j.
Tr. digitalis, ♏x.
Aquam ad ℥j.
t. d. s.

He took this for six weeks, having twelve fits in that time. The bromide was then increased ten grains, and in the following week he had two fits. He was ordered a five-grain zinc pill in addition. He had two fits the next fortnight, but the zinc made him vomit, so had to be reduced to two grains, and he got twenty-five grains of bromide of potassium instead of the half-drachm of bromide of sodium. In the following fortnight he had thirteen fits; cannabis indicæ gr. $\frac{1}{6}$ was added to the pills. In the next fortnight he had six fits; the zinc was raised again to five grains, and he went for a fortnight without a fit. Towards the end of the following fortnight he had seven or eight fits in a few days, then he went a month with only two fits on one day. He went on pretty well from that time, (April, 1882), until last October, when he had fifteen fits in three weeks. I then ordered him—

R. Sodii nitritis, ℈j.
Aquæ, ℥j.
t. d. s.

In the following week he had nine fits; and the dose was raised to half a drachm. That week he had three or four fits, so the medicine was continued, but the next time he came he said he had had fifteen fits in the week, and felt sick and giddy after each dose of the medicine. He was therefore ordered—

℞. Pot. brom. gr. xxv.
Tr. digitalis, ♏x.
Aquam ad ℥j.
t. d. s.;

℞. Zinci oxidi, gr. iii.
Extr. cann. ind. gr. $\frac{1}{6}$.
Ft. pil. t. d. s.;

and in the following month no fits occurred.

Case III.—A. K., aged 32, took the bromides and zinc without benefit for two months; he had three or four fits daily. He was ordered—

℞. Sodæ bibor. ℈j.
Liq. arsen. ♏ii.
Aq. ad ℥j.
t. d. s.

He had eight fits in the next four weeks, and then went three weeks without a fit. A large carbuncle formed on his buttock, and he had nine fits in a fortnight. The carbuncle healed well under Mr. Furneaux Jordan's iodine treatment, and in the next month he had eight fits. An ill-marked psoriasis broke out on his legs, presumably due to the borax; the borax was stopped, and bromides substituted. The rash got well, but the fits became worse, and so the borax was resumed; unfortunately without good result: he had ten fits the first fortnight, six the second, two the third, and six the fourth. He was then ordered scruple doses of sodium nitrite, and in the following week had four slight fits; in the next fortnight he had ten or twelve fits, but had been taking half-doses of his medicine by mistake; in the next week he had three bad fits and resumed the bromides. In the following week he had seven fits, and was ordered to

have a seton put in the back of his neck. In the next week he had five slight fits.

CASE IV.—M. R., female, aged 24, had been taking bromides with zinc and cannabis indica pills for nine months, without cessation of fits, about one a fortnight. She was ordered—

℞. Sodæ bibor. gr. xv.
Liq. arsen. ♏iii.
Aquam ad ℥j.
t. d. s.

After this she was free from fits for four weeks, when she had three on one day. The dose was increased to a scruple, and she has gone eight weeks without a fit.

Case V.—A. J. S., aged 16, was under treatment by bromides for six months, having about a fit a week, but in the last week has had twelve fits. Ordered—

℞. Sodæ bibor. ℈j.
Liq. arsen. ♏ii.
Aq. ad ℥j.
t. d. s.

He went after this for nine weeks without a fit, when he had one in bed. He then relapsed, but was ordered to take his medicine every four hours, when the fits stopped entirely. The ends of his fingers desquamated while taking the medicine.

CASE VI.—G. C., aged 16, had been taking bromides for ten months with no benefit. He was ordered scruple doses of borax three times a day. He had been having about one fit a fortnight. In the following fortnight he was not so well, having two bad fits, and in the next fortnight he had several fits, and was so ill that he had to go to bed. He was ordered—

℞. Sodii brom. ℈j.
Tr. belladonnæ, ♏x.
Aquam ad ℥j.
t. d. s.

In the next fortnight he had two fits; a five-grain oxide of zinc pill was added. In the next fortnight he had no fits, and in the next only one; bromide of potassium was substituted for

the sodium salt, and he had no fits in the next fortnight. After this he had bad attacks now and then, but sometimes went for a fortnight without fits. About this time he went up to London to the Hospital for the Paralysed and Epileptic. His prescription which he brought away with him was, a drachm of bromide of sodium to be taken at bed-time, and half an ounce of inf. cinchonæ three times a day. For a little time he seemed better, and I continued this treatment, but the fits returned as badly as ever. I feared to take him off the bromides altogether, recollecting my experience with the borax, so ordered him—

℞. Sodii nitrit.
Pot. bromidi aa ℈j.
Tr. digitalis, ♏x.
Aquam ad ℥j.
t. d. s.

Since taking this he has gone for eight weeks with only nine fits, and thinks himself better.

Case VII.—H. B., female, aged 21, has been attending as an out-patient for years, taking bromides without benefit, having fits every day or two. She was ordered—

℞. Sodæ biboratis, ℈j.
Aquam ad ℥j.
t. d. s.

She was better on this treatment for about five weeks, when the fits returned, and she was ordered to take her medicine every four hours. I have no further notes of this case, and presume she ceased attending.

I think there is evidence of decided benefit from the use of borax in Cases IV., V., and VII. Sodium nitrite has done good definitely in no case; doubtfully in Case VI.

Dr. Gowers speaks in favour of the administration of iron together with specific remedies, and gives some statistics in support of this opinion. On the other hand, Dr. Hughlings-Jackson and Prof. Brown-Séquard oppose this practice. I am certain that I have seen cases made worse by iron, and I think its routine administration very undesirable.

Although there can be no doubt of the power possessed by

the bromides to control the fits, they appear to be quite useless to stop the minor attacks of epileptic vertigo, which are often by their frequency more distressing to the patient than the graver but rarer convulsions.

I have found that caffeine and theine, which I had previously discovered to possess the power of relieving the vertigo of Bright's disease, are very useful remedies in this condition. I have also found benefit from the use of nitro-glycerine. The following cases are illustrations :—

Case VIII.—John D., aged 27, had been epileptic for seven years before coming under observation in August, 1881. His fits were readily stopped by ten-grain doses of bromide of potassium combined with digitalis, but he suffered much from vertigo. He was given theine, and the dose was gradually raised to three grains three times daily, with great benefit, the vertigo ceasing entirely. In course of the treatment the theine was twice discontinued, but its use had to be renewed on account of the recurrence of the old symptom.

Case IX.—John S., aged 20, had never had fits, but was subject to attacks of vertigo, in which he often fell down and lost his consciousness for a moment. The attacks had occurred about every week for the last two years. After taking bromide and digitalis without benefit, he was ordered one grain of theine three times a day, and under this treatment the attacks of vertigo ceased entirely.

Case X.—A. C., male, aged 19, got rid of his fits under the use of bromides, but remained very subject to vertigo ; on adding two grains of theine to his medicine the attacks of giddiness ceased.

Case XI.—Lizzie T., aged 21, obtained cessation of her fits by the use of fifteen grains of bromide of potassium combined with ten minims of tincture of digitalis taken three times a day, but was much troubled with frequent vertigo, which was not benefited by dieting and attention to the bowels, or by rhubarb and soda or caffeine in doses of two grains three times a day. On putting her on minim doses of nitro-glycerine three times a day, she was at once relieved.

Case XII.—M. S., female, aged 19, has had fits for five years every month. Under the use of bromides the fits were

effectually controlled, but she complained much of frequent attacks of giddiness, which were not at all relieved by two-grain doses of theine. I substituted for it minim doses of nitro-glycerine, which were raised afterwards to two minims, and on this treatment she remained quite free from giddiness.

The principal points to which I desire to draw attention are—

1. The value of combining bromide salts with each other and with digitalis.

2. The value of zinc and cannabis indica as adjuvants to the bromide.

3. The use of borax in some cases which resist the bromides.

4. The employment of caffeine or theine and nitro-glycerine in the treatment of epileptic vertigo.

ABSTRACT OF A CLINICAL LECTURE ON SYPHILITIC GUMMA OF THE PHARYNX.

BY R. CLEMENT LUCAS, B.S., F.R.C.S.,

Senior Assistant-Surgeon to Guy's Hospital, and Surgeon to the Evelina Hospital for Sick Children.

Two well-marked examples of gumma of the pharynx have presented themselves among my out-patients during the past year; one in a woman, the other in a man who is still occasionally to be seen attending for medicine.

In the woman's case the disease had so far advanced, and the local impediment to swallowing had so nearly caused complete obstruction, that she was brought to the very verge of the grave, and I do not believe that there is any disease other than syphilis which could have brought a patient so low, and yet be capable of complete relief by medicine. Those who may remember her when she first came tottering into the room with support, emaciated to an extreme degree, and almost unable to articulate owing to obstruction to breathing, would scarcely recognise the woman who less than a year later walked three or four miles to show me how completely she had recovered, and in what good health she continued.

When she was seated in front of us, pointing to her throat, her distress of breathing was so great that it was clearly eminently dangerous for her to be a moment out of reach of surgical aid, and I insisted on her coming into the hospital at once as an in-patient. On looking into her mouth, we could see a large swelling covered with mucous membrane, blocking up the pharynx and pressing forward over the top of the larynx, having the appearance of a post-pharyngeal abscess. When I pressed upon this swelling with my finger I found it somewhat soft, but not impressible and elastic as a collection of pus would have been. The examination excited a fit of coughing and dyspnœa of the most urgent and distressing kind, such as to

suggest that laryngotomy might be forthwith required, yet we found her most reluctant to enter the hospital. This, however, we prevailed upon her to do, and when we had an opportunity of examining her in bed we fell upon evidences of disease which rendered the diagnosis absolute and indisputable.

Now this woman, who was fifty years of age, denied all knowledge of any syphilitic infection, and for aught I know it might have been thirty-five years since she was infected, for there appears no limit to the age at which a gummatous relapse may occur in a person once infected, badly treated, and later depressed in health. She knew nothing of any skin eruption, prolonged sore throat of an ulcerative kind, inflammation of the eye, falling off of the hair—symptoms which one may look back upon as indications of the secondary stage. Her history is, nevertheless, a suspicious one. She left her home at the age of fifteen to marry a sea-captain, by whom she had five children, all of whom died in early infancy, from a few weeks to a few months after birth, with one exception, a child which lived to the age of three and died of convulsions. I am inclined to attribute the syphilis (from which she was undoubtedly suffering) to this alliance, and I would have you consider, in tracing the origin of any disease, how far occupation may be ground for fair suspicion. The sea-captain died after nine years, and she married a pilot within twelve months of his death. No children resulted from this marriage, and she lost her second husband three years ago. She has been married a third time about twelve months.

It was between her second and third marriage that her health appears to have broken down and she became subjected to gummatous swellings and ulcers. Of these she reluctantly yielded a history, when they were discovered after she was in bed. It is probable that the poverty she experienced at this time was the cause of the mastery which the disease then obtained over her constitution.

Two or three years before we saw her she began to suffer from rheumatic pains in her legs and shoulders, and eighteen months before that time a lump formed beneath the skin on the upper part of her chest, which broke down into a sore. Soon after, a swelling formed below the left knee, which after

four or five months broke into sores. Later a swelling formed on the outer side of the left arm above the elbow, which broke down into two sores. These when seen were circular, surrounded by great induration and with yellowish leather-like sloughs in their centres. A similar swelling occurred on the corresponding part of the right arm a little later, which also formed a sore. The tendency to form symmetrical sores on the arms, noted in this case, is opposed to the view enunciated by Mr. Hutchinson in his address on Syphilis before the Pathological Society, that a lack of symmetry is characteristic of the tertiary manifestations. Rather it is probable the lack of symmetry frequently observed is accidental, and dependent, as Dr. Moxon suggested, on the paucity of the eruptions, whereas secondary spots being abundant become thereby of necessity symmetrical. A large gummatous swelling had also formed on the inner side of the left arm just above the condyle. A scar over the right clavicle, causing the skin to be adherent to the bone, first attracted my attention, and a similar circular depression over the sternum indicated the position of another node which had suppurated.

About Christmas, 1879, she first began to experience difficulty in swallowing, but this was not accompanied by sore throat. She was noticed also to snore badly during sleep. The difficulty in swallowing gradually increased, in consequence of which she became much emaciated, and for some weeks had been unable to swallow solid food. She also experienced difficulty in breathing, and any slight exertion calling for increased respiration excited a croupy noise and sense of suffocation. A surgeon had on one occasion attempted to lance the swelling in her throat, but only blood escaped.

She came into the hospital on the 11th of October, 1880, but only remained four days, and then again attended as an out-patient. She was placed at once on large doses of iodide of potassium, and black-wash was applied over the open sores. This treatment acted like magic, and even before she left the hospital she had begun to experience relief. The open sores rapidly healed, and the gumma in the pharynx, which had been gradually forming for nine months according to her history, was completely absorbed; as were others which had not broken down before the treatment was commenced. No more striking

instance has ever come under my notice of relief from imminent danger by the simple administration of a drug.

The other patient was a man, fifty-three years of age, who came in July, 1881, and directed my attention to a swelling, the size of an egg, beneath the angle of his jaw on the left side. This was very hard, adherent to the surrounding structures, and very suggestive of a malignant growth. Searching for some initial lesion which might have given rise to a glandular tumour, we found in the back of his pharynx, on a level with the surface of his tongue, a large excavated ulcer with a grey surface and thickened edges, which had caused him considerable difficulty in swallowing. Now this irregular ulcer, with its secondary glandular induration, might very readily have been mistaken for epithelioma, but the site of the ulcer was not a favourite one for that disease, and excited in my mind other suspicions. On inquiring as to his state of life, he said he had been married twenty-four years, and that he had seven children and his wife still living. This statement might have put us off our guard had we not been attracted by a desquamative condition of his finger-tops and an indurated spot and scar or two upon his face. When questioned directly as to syphilis, he admitted that six years previously he had acquired the disease, which was followed by the usual train of secondary symptoms, sore throat and eruption. He was intermittingly treated by several private doctors or chemists during the first two years after infection. He then came out in large blotches over his trunk and extremities which formed sores (rupia), some of which left scars. He next went to St. Thomas's Hospital, and afterwards to the Skin Hospital, where he was treated, as his letter showed, by arsenic. In 1879 he went to St. Bartholomew's Hospital, where he was treated with the green idode of mercury pill and arsenic, and got better, but the palms of his hands and fingers began to desquamate and have continued to do so since. This squamous syphilide of the palms, or syphilitic psoriasis, as it is often called, appears to commence as an intermediate or late secondary eruption running on into the tertiary period. Its favourite site is in the hollow between the thumb and forefinger, and it commonly attacks both hands. It is characterised by an injection of the derma with thickening of the epidermis, followed by

cracks, which plough up the epithelium so as to leave a roughened surface. It spreads from one or several spots and creeps gradually over the palms and fingers. Reaching the finger-tops, it will often cling persistently about the bases of the nails, causing them to be shed or be imperfectly developed.

In our patient's case the condition of his hands was an important factor in determining, apart from history, the disease from which he was suffering.

Another symptom of which he complained, was persistent headache, worse always at night, and on feeling over his head, we found a tender, slightly raised lump, evidently a node, on his left parietal bone. He was treated by the internal administration of ten grains of iodide of potassium three times a day, and the blue mercurial ointment was rubbed in over the gland. The ulcer in his pharynx rapidly took on a more healthy appearance, and at the end of a month was completely healed. His finger-tops and hands were treated at the same time with the ammoniated mercury ointment. In October, 1880, scarcely any scar could be seen in his pharynx, and the swelling in his neck which had been undergoing gradual absorption was reduced to the size of a walnut. In December the gland was about the size of a nut, and his general health had greatly improved. He had completely lost the headache and tenderness of his scalp. In February, 1881, the gland was reduced to the size of a pea, and his hands and finger-tops were almost well.

Gumma of the back of the pharynx is not common, and I know of no recorded case similar to that of the woman, where a syphiloma has formed in this situation of such magnitude as to cause almost complete obstruction to deglutition, and to interfere seriously with respiration. The diagnosis would have been difficult or impossible, had not the presence of gummata in the cellular tissue and nodes on the bones thrown light on the nature of the swelling behind the pharynx. It is not to be wondered that those who first saw her attempted to give relief by incision, under the impression she was suffering from post-pharyngeal abscess.

There are two other tumours in this situation with which this syphiloma might have been confounded, viz., fatty tumour, and aneurism of the internal carotid artery.

A case, which I believe to be unique, of fatty tumour com

mencing in the cellular tissue, behind the pharynx, came under my notice at the Evelina Hospital, about five years ago. It occurred in a child, æt. 4, who was afterwards admitted into the hospital under the care of Dr. F. Taylor, where it died from the continued effect of obstruction to deglutition and respiration. In addition to the projection into the pharynx, which resembled an abscess, there was an enlargement of the neck which rose and fell with the larynx like an enlarged thyroid gland. This was after death ascertained to be the lateral portions of the tumour, which were wrapped round the sides of the larynx and moved with it.[1]

Aneurism of the internal carotid is by no means common, but an instance has come within my knowledge where the tumour projected into the pharynx, and caused great dysphagia. The fact that it pulsated, and that it was reduced in size by pressure upon the common carotid, were sufficient to distinguish it from other tumours in this situation.

Lastly, one ought to say a few words on retro-pharyngeal abscess, since it is with this that the rarer tumours in this region are liable to be confounded. After one of the exanthemata of children, it would appear to occur occasionally independent of bone disease, and, though very dangerous, is not necessarily fatal. Most commonly, it is associated with disease of the bodies of the second or third cervical vertebræ, some indication of which is often found in a thickening of the back and sides of the spine; but what is most characteristic is the fixed attitude assumed by the child, and its inability to turn the head. It is in these cases alone, where disease exists in the cervical spine, that I think it necessary to warn you against suspension of the patient by means of pulleys during the application of a plaster jacket. There is an ever-present danger, when the axis or the lateral mass of the atlas is diseased, that the transverse ligament may yield, and the patient be suddenly killed by the pressure of the odontoid process upon the cord. Much was written of the danger of suspension, in any case, when Sayre's treatment was first introduced, but we have suspended many hundreds of cases for disease in the dorsal and lumbar regions at Guy's and the Evelina hospital without, so far as I am aware, ever occasioning any ill effect.

[1] *Transactions of the Pathological Society*, vol. xxviii., p. 216.

Reviews.

A Dictionary of Medicine, including General Pathology, General Therapeutics, Hygiene, and the Diseases peculiar to Women and Children. By Various Writers. Edited by RICHARD QUAIN, M.D., F.R.S., Fellow and late Senior Censor of the Royal College of Physicians, &c. 8vo, pp. 1816. London: Longmans. 1882.

IN a subject which is advancing so rapidly as scientific medicine, it becomes a necessity that something like a stock-taking should be held from time to time. No one mind can grasp the daily additions in fact and doctrine. A concurrence of authorities, each reporting on his own subject, is needful. This dictionary is the outcome of an able effort to supply the necessity. It is more than a book of definitions; it is a treatise on systematic medicine, in which the articles on the more important subjects constitute monographs in themselves. We can best compare it to Wagner's *Handwörterbuch,* which was, on its appearance in Germany nearly forty years ago, both a summary and a starting-point for scientific physiology. The plan of obtaining men possessed of special knowledge to write upon each subject and thus to secure greater excellence in a work on medicine has already been adopted by Reynolds in this country and by von Ziemssen in Germany in their *System and Cyclopædia of Medicine.*

The difference between these works and the present one is expressed in their respective titles. The former are systems containing in each section the diseases of some organ, with their etiology, symptoms, duration, and treatment, discussed together in a manner suitable for imparting systematic information. The present is a dictionary, in which the subjects, which in the others are grouped together, are, in many instances, to be found under numerous headings, arranged in alphabetical order, and so available for ready reference. While the other works are more adapted for systematic study, the present is more convenient for simply refreshing the memory on single points.

Dr. Quain has been peculiarly happy in securing the aid of so many who are acknowledged to be past masters in their own

departments as well as of two such able and accomplished assistant-editors and fellow-labourers as Dr. Mitchell Bruce and Dr. Frederick Roberts. As Dr. Quain acknowledges in his preface, it would have been impossible without their help to have fulfilled the editorial duties he undertook in attempting to bring out a work like the present, and to them also must be awarded some of the credit of their successful accomplishment. There are some hundred and seventy contributors, and all of them bear names that are well known to the profession. There are moreover evident signs of the careful editorial supervision necessary to unify the work of so many different hands, and to preserve the due proportion of the various subjects. Such unity and proportion are the qualities hardest to attain in an encyclopædia; it is therefore creditable that the editors have achieved so much, and not surprising that on this point they have come short of perfection. As a single instance we may refer to Dr. Bastian's inadequate article on *Bacteria* generally, compared with Mr. Simon's on *Contagion*, Dr. Greenfield's on *Micrococci and Malignant Pustule*, Mr. Horsley's on *Zyme*, *Bacilli*, &c., and Dr. Bishop's on *Antiseptics*. Dr. Bastian's views on the general question would hardly prepare the reader for the conviction of the importance of micro-organisms which pervades the special articles. But Dr. Bastian more than makes up for this shortcoming by his article on the *Diseases of the Spinal Cord*, which is all that could be wished.

We have noticed one dangerous misprint, which should be corrected at once: on p. 1181, col. 2, line 9, the dose of picrotoxine is given as "$\frac{1}{6}$" of a grain; this should clearly be "$\frac{1}{60}$."

For years to come "Quain's Dictionary" will be a classical work of reference, and we only do our plain duty in commending it to the notice of all practitioners. There is no other single volume that can take its place; and a shelf-full of special treatises would for most of the purposes of daily practice be less useful because less compact and less easy of reference.

A Treatise on Comparative Embryology. By FRANCIS MAITLAND BALFOUR, LL.D., F.R.S., late Professor of Animal Morphology in the University of Cambridge. 8vo. 2 vols. London: Macmillan and Co. 1881.

FEW can have failed to notice and be impressed by the rapid advance of embryology during the last eight or nine years, and the manner in which it has forced itself to the front and compelled the attention and respect of all. Von Baer's great work on the *Development of Animals*, the foundation of modern embryology, was completed in 1837; but, in this country at least, it was not until the publication, in 1874, of the *Elements of Embryology* by Foster and Balfour, that the new science

received general and serious attention. In the eight years that have elapsed since that date, embryology has made astonishing progress; an enormous number of facts have been accumulated, far-reaching theories formulated, and, more important still, the position and dignity of the science has been thoroughly established and recognised.

Nor is it difficult to understand the interest which attaches nowadays to the study of embryology, and the respect which it everywhere commands. The clue is to be found in its intimate relation to the great generalisation which more than any other has occupied men's minds for the last five-and-twenty years. This connection between embryology and the Darwinian theory is of a twofold nature; on the one hand, embryology affords invaluable aid in determining the genealogical relationship which the doctrine of evolution tells us must exist between different animals and groups of animals; on the other hand, the doctrine of evolution affords a full and complete explanation of innumerable facts and details in individual development, which otherwise would have no value or meaning.

Who, for instance, but for the theory of evolution could understand why a dog, or a human being, during the early stages of its development should have gill-slits in its throat exactly similar to those of a fish, but never employed in any way, and doomed to disappear long before it is born? Who, again, but for embryology could understand the zoological affinities or the structure of an ascidian or a barnacle? or such features in human anatomy as the course of the recurrent laryngeal or chorda tympani nerves, the presence of the fossa ovalis and Eustachian valve in the heart, the existence of the ductus arteriosus, or of the round ligament of the liver, the presence of ventricles in the brain and of the central canal in the spinal cord, the existence and relation of the processus gracilis of the malleus, the distribution of the sixth nerve to the external rectus muscle? or such abnormalities as coloboma iridis, the substitution of a right for a left aortic arch, intestinal diverticula, cleft palate, spina bifida, or teratoid tumours? Embryology, indeed, as a science owes its importance, if not its very existence, to its close association with the theory of evolution; and it is to the clear recognition and enunciation of this connection that the charm as well as the value of Balfour's work is in large measure due.

The difficulties of writing such a work as the *Treatise on Embryology* were enormous. An almost incredible number of observations had been recorded and memoirs published by different observers, working in different countries, by different methods, and often in direct antagonism to one another; conflicting statements were made by men of equal authority;

theories, sometimes well founded, but more often crude and inconsistent, were put forth in startling numbers, and their truth assumed without any adequate proof being advanced. Add to this that the development of many groups of animals was absolutely unknown, and some idea can be formed of the task which Balfour set himself to accomplish when commencing his great work. By no one were these difficulties more keenly felt and appreciated than by Balfour himself. In his preface he expresses a fear that the attempt to grapple with them may be held by many to be "premature, and therefore doomed to failure;" and adds, "I must leave it to others to decide how far my effort has been justified." Concerning this verdict there cannot be a moment's doubt. By the consent of all who are in any way qualified to judge, the *Treatise on Comparative Embryology* is one of the most valuable contributions ever made to biological literature, and its publication will long be remembered as marking one of the most important epochs in the history of biological science.

Of the two volumes of which the treatise consists, the first opens with three introductory chapters dealing with the formation of the ovum and the phenomena of impregnation and segmentation. These important subjects are dealt with in a masterly and characteristic manner. An immense number of observations have been carefully sifted, all of good they contained picked out, and the whole cemented together into a connected and intelligible theory. The remainder of the volume is devoted to a systematic description of what is known concerning the embryology of the several groups of invertebrates. Difficulties, which from the imperfect condition of our knowledge must be numerous, are always clearly stated; points of theoretical interest, such as the ancestral value of the *nauplius* or *zoea* stages of *Crustacea*, are expounded and discussed at length.

The second volume is of even greater interest and importance, for more reasons than one. In the first place it deals with vertebrates; and the embryology of vertebrates is much better known, and forms a more consistent whole than that of invertebrates. Moreover, nearly all embryological facts of primary importance can be learnt from them more readily than from invertebrates. Additional interest attaches to the second volume, from the fact that a very large amount of the ground is covered by Balfour's own work. The chapters on the lampreys, elasmobranchs, ganoids, and lacertilia are almost entirely, and other chapters very largely, based on his own researches. The systematic description of the several vertebrate groups occupies about one-third of the volume. Then follow chapters of extreme interest on the early stage of development of vertebrates, the ancestral form of the chordata, the origin and

homologies of the germinal layers, and on the nature, origin, and affinities of larval forms. These chapters are, in our opinion, at once the most powerful and the most attractive of the whole work; they deal, in a remarkably philosophical and at the same time fascinating manner, with the burning problems of embryology, and form the most able exposition yet written of the true methods and aims of the science.

Had Balfour stopped here the treatise would have been complete in itself, and there could have been no complaint on the score of omission. Fortunately he did not do so. The second half of the volume, more than three hundred pages in length, is devoted to organogeny, *i.e.* to a systematic description of the principal developmental modifications assumed by the several organs of the body in the different groups of animals; the arrangement being a morphological one as contrasted with the zoological method adopted in the earlier part of the work. To many, perhaps to most, the Organogeny will be the most valuable part of the whole work. The several organs and systems are taken one by one, and their developmental history traced throughout the several zoological groups, more stress being for obvious reasons laid on vertebrates than on the more imperfectly known invertebrates. So thoroughly is this done, in so comprehensive and impartial a manner are the views of others expressed, and so much original matter of extreme value do these chapters contain, that it is hardly too much to say that the addition of the Organogeny has rendered the *Treatise on Comparative Embryology* the best work extant on animal morphology, besides being the only complete work on the subject more immediately expressed by the title.

This part contains chapters on the epidermis; on the nervous system, containing a summary of his own most important work, which has almost revolutionised our conception of the vertebrate nervous system; on the optic, auditory, and olfactory organs; on the vertebral column and skull; on the limbs and limb-girdles, containing a clear account of his own attractive theory as to the nature of the vertebrate limbs, according to which they are to be viewed as formed from primitively continuous lateral fins; on the vascular and muscular systems; on the excretory and reproductive organs and their ducts, a subject which engaged his attention in a special manner; and on the alimentary canal and its appendages.

If we add to this, that the work is well illustrated throughout, a large number of the figures having been specially drawn for it, and that it is provided with very full and useful lists of reference to the chief papers and memoirs bearing on the several portions of the subject, we have said all that is necessary to show that no pains have been spared to render this remarkable book

invaluable to other investigators, and in all respects worthy of its gifted author.

We may, indeed, in our yet unhealed sorrow at his sad and most untimely death, find some consolation in the thought that he lived to complete his great work, and that he has left us in it not only an invaluable record of work done, but also a rich store of fruitful suggestions of work yet to be accomplished. It would have been a noble memorial if produced at the end of a long and active life—it is a stupendous achievement for a man who had but just completed his thirtieth year.

The Physiology and Pathology of the Blood. By R. NORRIS, M.D., F.R.S.E. London: Smith, Elder and Co. 1882.

THIS is a large book, richly illustrated with microphotographs. Essentially it treats of the existence, life-history, functions, etc., of certain corpuscles which Dr. Norris asserts exist in normal blood. They are different both from the red and from the white corpuscles, and so form a third morphological element of the blood.

The new corpuscle has, according to the author's own description and his photographs, the most exact resemblance to the stroma of a red corpuscle. By the stroma we mean, of course, the solid substratum of the corpuscle which is left when the colouring matter is removed. This being the case, it is obviously necessary that the methods employed for demonstrating the new corpuscle should be such as to exclude the possibility of stromata being formed. Now so far is Dr. Norris from perceiving the importance of this essential condition that he adopts methods which are particularly well calculated to give rise to stromata; and there is in fact no doubt whatever that what he has seen and photographed and described are simply stromata, and nothing else. Dr. Norris himself admits indeed that one of his methods (the saturated salt-solution method) gives rise to solution of hæmoglobin. The method of rendering the blood ice-cold and then fixing the corpuscles, and of adding absolute alcohol to blood in the way he describes, is also well adapted to give rise to stromata. Lastly, in his description of the photographs, he talks in the same sentence of his invisible corpuscles and of a highly coloured serum. The inference should have forced itself upon him that the two things were connected.

We have said that, if solution of hæmoglobin take place, stromata will be produced. Dr. Norris states, however, that, by the use of "dry soluble albumen, the hæmoglobin is absolutely prevented from leaving, and that the invisible corpuscle is still present." This is, he thinks, a complete answer to an objection such as we have taken. He gives photographs of blood treated in this way, but it is impossible to discover in them any such

bodies as are depicted in Fig. 25 and elsewhere, where the so-called "invisible" corpuscles are distinctly to be seen.

In all cases where the invisible corpuscles are distinctly to be seen in his photographs, we have no hesitation in pronouncing them to be stromata. No one who has had any experience in working with blood would, we think, take any other view of them. With such methods as the author uses, it is impossible for him to eliminate this simple alternative explanation of his results.

Mrs. Ernest Hart, in the *London Medical Record* for October, 1882, has fully discussed Dr. Norris's researches. She has carefully repeated his experiments, and comes to the conclusion that his corpuscles are nothing more nor less than decolourised red corpuscles, *i.e.* stromata. This experimental examination of the question altogether confirms the opinion we have formed from consideration of the methods and the photographs. We can but regret that so much earnest labour should be spent by Dr. Norris in maintaining an indefensible position.

A Supplementary Catalogue of the Pathological Museum of St. George's Hospital: a Description of the Specimens added during the Years 1866—1881. By ISAMBARD OWEN, M.D., Curator. London: J. and A. Churchill. 1882.

IT would hardly be fair to judge a medical school entirely by the wealth of its museum, but the capacity of keeping it in good working order and providing a catalogue at once practical and thorough is valuable evidence of sound vitality, for the work is not easy, and seldom receives full thanks. The book before us is the continuation of a large and excellent catalogue compiled in 1866 by Mr. Timothy Holmes and Dr. J. W. Ogle, and shows very careful work. The descriptions are concise, and can only bear one interpretation. They do not deal so much with what could once be made out in the fresh specimens by free manipulation as with their actual appearances as they can now be seen, which is just what is necessary for the genuine student of museum pathology. A short clinical history is given wherever obtainable, and in the majority of cases, viz. those taken from the Hospital practice, this has been drawn up *de novo* from the abundant official clinical records in the possession of the Hospital.

ERRATUM.—In the notice of Dr. Little's book on *Genu Valgum*, in the last number (p. 40, line 23 from top), *for* "by its use," *read*, "during its use." The reviewer desired to emphasise the importance of using means to avoid over-extension as well as flexion during the treatment of genu valgum by splints.

Clinic of the Month.

Manganese in Amenorrhœa.—Drs. Sydney Ringer and Murrell write to the following effect: "We are desirous of calling attention to the value of a very simple remedy in a very common complaint. For some time past we have used permanganate of potash with much success in the treatment of certain forms of amenorrhœa, and are satisfied of its value. Our observations have extended over a period of thirteen months, and we have now notes of sixty-nine cases. The majority occurred in hospital practice, but some were private patients. A small number remained under observation for a few weeks only, but the majority continued to attend for a much longer period; so that in some instances we have a complete record of the menstrual history for a year or more. In some cases the amenorrhœa was the cause of the patient seeking advice; in others its existence was mentioned incidentally, the patient suffering from some other complaint. Our cases are such as come under the care of the general as distinguished from the obstetric physician, and do not include those requiring operative interference. As a rule we refrained from making a vaginal examination, but with this exception our notes are complete. We have used the permanganate in two forms, first the pharmacopœial solution, and secondly the permanganate made into pills, each containing either one or two grains. Generally we begin with a grain three times, and then gradually increase the dose to two grains four times, a day. Our most striking results have been obtained with the larger doses; a large dose sometimes succeeding admirably after the failure of a small one. Before commencing treatment we inquire carefully into the menstrual history of the patient, and as a rule give the remedy only for the three or four days immediately preceding the expected period, but should it fail to produce the desired effect we direct the patient to continue steadily taking it, and in some cases it has been taken continuously for nearly three months. In our experimental observations we have given the one drug only and have done nothing in the way of accessory treatment. Our most striking results have been obtained in young women between the ages of eighteen and twenty-five, who

from some accidental or trivial cause, such as catching cold or getting wet, have 'missed' once or twice after having been regular. The administration of one or two grains of permanganate of potash in pill three or four times a day for a few days before the time of the expected period will bring on the flow almost to a certainty. In some instances the periods were brought on after the patient had ceased menstruating for over a year. In the case of country girls who have 'seen nothing' for a month or two after coming to town the treatment has answered admirably. Often enough patients do not consult their doctor until they are 'overdue,' until the time of the expected period has passed by for some days. Even then the prompt administration of the permanganate will often bring on the flow at once, but should it fail to do so the treatment ought to be continued, and the patient will probably menstruate normally at the next monthly time. Generally our efforts are not crowned with success until the medicine has been taken for at least three or four days, but in some instances the permanganate acted with striking rapidly, the menstrual flow making its appearance after only two or three doses had been taken. It is not necessary to discontinue the treatment on the appearance of the menses; in fact we generally tell the patient to continue taking the pills three or four days longer, finding that it facilitates the flow. The permanganate often succeeds well after the failure of other remedies, such as iron, aloes, nux vomica, strychnia, pulsatilla, nitro-glycerine, and hot mustard baths. Sometimes, however, it is necessary to give it for six weeks or even longer before the desired result is obtained. In those cases where the patient has menstruated only one or twice, and has then entirely ceased for some months, our treatment answers well; the menstrual function is re-established, and thenceforth proceeds normally at every successive monthly period. In some cases there was no actual amenorrhœa, but the flow was scanty, lasting perhaps only a single day, or it may be only a few hours. Here the administration of the permanganate prolonged the flow, and even in some instances when it had ceased brought it on again. In girls of about fifteen or sixteen, who have never menstruated at all, the permanganate, as might be expected, is not so certain in its action; but even here it not infrequently acts promptly, bringing on the flow at once. In some cases where the general health was bad, and the permanganate had failed, we suspended treatment for a time, and sent the patients into the country for a month. On their return we gave the permanganate a second trial, and it succeeded at once. We have, however, sometimes failed to bring on the menstrual flow even when the patient was in fairly good health, and when there were the usual indications of puberty.

It is not only in the case of young women that manganese is so useful, it succeeds almost equally well with women between thirty-five and forty, who, as the result of many pregnancies and much suckling, have ceased to be regular. Here, for example, is a typical case. A married woman came to us complaining that she was never regular. She had had nine children in as many years, and rarely 'saw anything' more than once between her pregnancies. She had been suckling for eight months, and had not been poorly for seventeen months—the nine months she had carried and the eight months she had suckled. She was not in the family-way, but said she expected she would be soon if she weaned the baby. She did not know when she ought to be poorly, and had given up all expectation of seeing anything. She was ordered two one-grain permanganate of potash pills four times a day, and came on poorly a fortnight after, the first time for seventeen months.—We need hardly say that before treating the amenorrhœa care should be taken to see that the patient is not pregnant, although we are satisfied that the permanganate given in the dose we recommend has no power to produce abortion either in the early or late stages of pregnancy. We find that manganese fails to induce the flow when the amenorrhœa is due to advanced phthisis. But in some cases of arrested phthisis the treatment was successful, and the patient, after a time, under the influence of the permanganate, menstruated freely and at regular intervals. In several instances patients informed us that the pills had proved of value in curing 'whites' of long standing. As a rule the permanganate is taken without difficulty, but patients much prefer the pills to the solution. The solution is peculiarly disagreeable to take, and in some cases produces nausea and even vomiting. Patients frequently complained after taking the pills of a heavy persistent pain over the upper part of the sternum, 'as if something had stuck there and would not go down.' This was not due to the drug being given in the form of a pill, for the same complaint was made when the same dose was given in solution. One patient said the pain was of a burning character, and another said it was like heartburn. A girl of sixteen, to whom two two-grain permanganate of potash pills were given four times a day, said the pain, 'like a lump at the chest,' came on immediately after each dose, and was so intense that she had to go to bed for two hours. That the effects we have described are due to the manganese, and not to the potash in the salt, is shown by the fact that manganate of soda and binoxide of manganese are equally efficacious in the treatment of amenorrhœa. The manganate of soda was given in two-grain pills, two four times a day; and the binoxide in four-grain pills, one four times a day. It may be thought that the manganese acts by improving the

condition of the blood, but this is not the case. The treatment succeeds equally well in the plethoric and in the anæmic. Given in cases of chlorosis, the permanganate not infrequently brings on the period without in any way improving the anæmia." (*Lancet*, Jan. 6, 1883.)

Diabetic Coma.—Drs. Foster and Saundby, of Birmingham, report a case of diabetes in a patient aged 17. The disease ran an unusually rapid course, and the day after admission to the General Hospital the patient, who up to then had been going about, fell into a comatose state, and died in a few hours. The autopsy showed that there was no fatty embolism. The authors sum up the lessons of the case thus: (1) Diabetic coma is specially apt to occur in acute cases in young persons. (2) Diabetic patients and their friends should be warned of the danger of constipation, muscular exertion, nervous excitement, and cold, as probably predisposing causes of death by coma. (3) The discovery of the ferric chloride reaction (a deep vinous red colour with the urine) should be taken as a warning to expect the onset of coma. (4) Deep respiration, rapid pulse, and abdominal pain are the earliest premonitory symptoms of diabetic coma. (5) Cyanosis may be absent though dyspnœa is present, and may appear only just before death. (6) Convulsive seizures are not uncommon just before death. (7) Diabetic coma, with all its classical symptoms, occurs independently of any excess of fat in the blood, and the pathological significance of lipæmia when present is as yet undetermined. (8) The toxæmic theory, viz. that of poisoning by acetone or some nearly allied substance or substances, affords the best explanation of this remarkable group of symptoms. [*Practitioner*, xxx. p. 49.] (*Birmingham Med. Rev.*, Jan. 1883.)

The Diagnosis of Pulmonary Syphilis.—An abstract of an alleged case of pulmonary syphilis has recently been given by Dr. Güntz. The previous history of the man showed that two years after infection an eruption appeared on the skin, and a year later cutaneous ulceration was noted; five years after infection the lung trouble was first noticed. The left lower lobe was affected with a circumscribed infiltration, the symptoms being cough and shivering. The dulness to percussion had not disappeared after a period of eighteen months' good general health, at the end of which the patient began to spit blood. This was soon followed by an increase in the size of the infiltrated area. For six days the expectoration consisted of chocolate-brown lumps; later, muco-purulent sputa were brought up. The pulse was 90; the breathing 26 to 32 per minute; but there was no fever. The physical signs underwent no appreciable change; there was dulness and pectoriloquy with

some râles. The sputa were hardened in alcohol, and became tough and membranous — some, nevertheless, were lighter than water. The microscope revealed a fibrillated stroma, with finely granular débris, old and young cells and nuclei, here irregularly scattered, there arranged in groups. No pulmonary tissue or vessels were detected. Some sputa were sent to Lancereaux, who also regarded the microscopic elements as of a gummatous nature. (*Wiener medizinische Wochenschrift*, No. 46, 1882; *Med. Times.*)

Dry Gangrene from Local Application of Carbolic Acid.—Dr. J. B. Garrison relates the following case in the *Western Med. Reporter.* About the middle of February last, a daughter of Dr. Childress consulted her father as to an onychia in process of development on her right index finger. She was directed to apply carbolic acid; but, instead of applying a few drops to the affected part, she wrapped the entire finger, as far as the second joint, with several folds of linen, and poured on it to saturation pure liquefied carbolic acid, and allowed it to remain all night. Next morning the bandage was removed; and, on the third day after the occurrence, when Dr. Garrison first saw it, the finger, as far as the second joint, was as black as jet, cold, perfectly anæsthetic, wrinkled and shrivelled, with sulci apparently clinging to the bone; it was hard as wood; in a word, actually mummified, with a line of demarcation entirely around the finger, indicating a complete separation of the dead from the living tissue. Athough there seemed no possibility of saving the finger, as it had actually lost every vestige of vitality, he directed a small rubber band to be tied round the finger, near the metacarpo-phalangeal articulation, sufficiently tight to obstruct the reflux of venous blood without repressing the arterial supply. This was applied for five or ten minutes every hour, and kept up continuously for more than two months. The tissues of the finger gradually yielded to the mechanical pressure of the blood, and the digit resumed its shape and functions, except that it was entirely denuded of integument. The old skin was allowed to remain as a protective, and warm moist poultices, with oil and glycerine, were constantly applied to soften the tissues. The fortunate result of this case is an additional argument in favour of the principle of conservatism in surgery, which should obtain in all similar cases. (*Lond. Med. Record*, Dec. 1882.)

Pulmonary Embolism as a Cause of Sudden Death in Phthisis.—M. Duguet reports a case of sudden death during the course of phthisis, in which he found a tough non-adherent clot obstructing the branch of the pulmonary artery supplying the left lower lobe. No source for the embolus could

be found in the vena cava or iliac veins, but in the right profunda femoris vein there was a clot about two inches long, which occluded only partially the lumen of the vessel. At its upper end it was irregularly torn. It is notable that there was no œdema of the leg, nor had the patient complained of any pain. (*L'Union médicale*, No. 130, 1882.)

Injection of Corrosive Sublimate in Syphilis.—This treatment has been used by Prof. Lewin in upwards of 50,000 patients at the Berlin Charité. While after Zittmann's treatment there were 90 per cent. of relapses, and after the inunction cure 80 per cent., after sublimate injection there have been but 30 to 35 per cent., these being moreover very benign. The advantage of the method is in the safety with which it can be applied. Abscesses are never produced, and the pain is trifling. The gluteal region is the part chosen for the injection. [*Practitioner*, xxix. 372.] (*Phil. Med. Times*, Sept. 23, 1882.)

Myositis ossificans.—At a recent meeting of the Vienna Medical Society Professor Podrazki exhibited a soldier affected with the rare condition which has been termed myositis ossificans. Four weeks previously the man had applied for treatment on account of an intense inflammation of the muscles on the front of the right upper arm, apparently set up by severe gymnastic exercise. The muscles were large, hard, and uneven, and the elbow-joint was fixed in flexion. The hardness was removed, and some increased mobility was obtained, by massage and the application of cold. At the end of two weeks a hard, round, movable tumour developed in the flexor of the elbow, which was evidently due to an ossification of the brachialis anticus. At first it was movable, the upper part appeared to be cartilaginous, and it was evidently not connected with the periosteum. Podrazki has seen, in the course of nineteen years, two cases in the practice of Pitha quite similar to this in their characters. In those two cases neither iodide of potassium, nor any other treatment adopted, had any influence. In a discussion which followed, Professor Weinlechner stated that he had twice seen similar small spots of ossification in the muscles on the front of the leg, due, in each case, to a traumatic cause. Kundrat expressed the opinion that some supposed exostoses on the thigh proceed from muscles. Their form and seat correspond to certain muscles. Their greater frequency in men, and especially in muscular individuals, suggests that their origin is traumatic. They constantly become adherent to bone in the course of their growth, and hence are commonly thought to be primary exostoses. (*Lancet*, Dec. 16, 1882.)

Duboisia in Exophthalmic Goitre.—M. Desmos narrates three cases of exophthalmic goitre treated by hypodermic

injections of neutral sulphate of duboisia. The quantity injected daily was from 0·5 to 1 milligramme, according as the patient could bear. Marked improvement resulted in each case: the projection of the eyes and the palpitations diminished; the general health improved; the thyroid enlargement with the accompanying pulsation and bruit decreased greatly. The improvement was checked when the treatment was left off. The patients have not yet been watched long enough to ascertain whether their recovery is lasting. Slight symptoms of poisoning, such as formication and cramp in the gluteal region, were noted in two cases and limited the amount of the drug administered. (*Rev. des Sciences méd.* Oct. 15, 1882.)

Hot Water in Epistaxis.—M. Auquier mentions a case in which he was called to a young man of twenty who had been suffering for three hours from violent epistaxis. The patient had been subject to such attacks from infancy. M. Auquier tried in vain to stop the bleeding by means of cold water, plugging the nares, mustard-plasters, &c. At last he irrigated the nose with very hot water, with instant success. During the next night and day the friends of the youth were able by this means to stop at the outset several fresh outbreaks. The author thinks the hot water acted by producing a reflex contraction of the bleeding vessels; and not by encouraging the flow and so causing depletion of the superficial vessels, as has been supposed, in reference to the stoppage of uterine hæmorrhage. (*Gaz. hebd. de Montpellier*, Nov. 4, 1882.)

Syphilitic Affections of the Eyelids.—Dr. Theodore Wiethe contributes three cases of syphilis of the eyelids under as many forms, viz.: (1) initial sclerosis of the lower lid, (2) ulcerated papule of the upper lid, (3) gumma of the lower lid. He gives the statistics of Zeissl, who, among 40,000 cases of syphilis, saw only eight of affections of the lid, and remarks that the oculist meets with but one undoubted case of syphilis of the lid in 10,000 eye patients. A full description of each case is given with references by comparison to the cases of Arlt, Hirschler, Desmarres, and Galezowski. Treatment consisted of iodide of potassium internally, inunctions of mercury and iodoform locally; recovery followed in each case. It is worthy of note here, that the microscopical examination of a section of the gumma taken from its matrix showed chiefly swollen fibrillæ of connective tissue. (*Arch. of Dermatol.* Oct. 1882.)

Scarlatiniform Erythema in the course of Acute Rheumatism.—At the meeting of the Société Médicale des Hôpitaux, held October 13th, M. Hallopeau reported a case of acute rheumatism in which there occurred an intensely red

livid eruption on the entire body, accompanied by chilliness and exacerbation of the febrile movement and malaise, but with no anginose symptoms; the temperature of the skin to the touch was greatly increased. This lasted one week and then desquamation commenced in the form of white scales on the face and in large patches on the trunk and limbs. Three days later, pericarditis occurred, accompanied by intense dyspnœa and pulmonary congestion, and proved rapidly fatal. At the autopsy there was found a marked thickening of the corneous layer of the epidermis and decided projection of the papillæ. The patient had had a similar attack accompanying an attack of rheumatism four years previously. (*Journ. de Méd. de Paris*, Nov. 4, 1882.)

Treatment of Chancroids with Iodoform. — In relating his experience in the Lock Wards of the Edinburgh Royal Infirmary, Mr. A. G. Miller says that his treatment of chancroids has been modified very much, and latterly simplified, and also, he is sure, much improved, since the introduction of iodoform as a remedy. He used to cauterise these sores, especially when phagedænic, destroying them with caustic potash or chloride of zinc. Now he simply dusts them with iodoform powder and keeps them dry, and they invariably heal up in a few days. Iodoform is especially useful in the female on account of its power of diffusing itself and penetrating into corners. Formerly, with the caustic treatment, he was never certain that he had destroyed all the sores, and knew that if one was left the chancroidal action would reproduce itself. Now he can be perfectly certain that if he puts on the iodoform freely the disease will be thoroughly checked. The action of iodoform on phagedænic sores is even more remarkable than on ordinary chancroids. His experience is that 24 hours, or at most 48, are quite sufficient to establish a healthy action in the sores. He uses the iodoform pure, the crystals being pounded to a fine dust, which is blown on to the parts affected by means of an instrument consisting of a wooden tube, widened out at the centre, where the powder is placed, and then blown out at the nozzle by pressure (with the thumb) on the india-rubber ball placed at the other extremity. When the labia are held aside by means of Dr. Henderson's forceps in the hands of an assistant, the surgeon can blow any quantity of the powder that may be necessary directly on the affected parts, and as these are always damp, a sufficient quantity of the iodoform adheres to destroy the septic action of the sores. If all the sores are not reached at first, a second or third application may be necessary. Generally there was sufficient dusted on to act on all the sores, even those that were out of sight. Chancroids on a syphilitic person run

an ordinary course, but are apt to be followed by condylomata.—(*Edinburgh Medical Journal*, Nov. 1882.)

Importance of Ear-affections in Diphtheria.—Dr. Bürkner reports two cases in which middle-ear inflammation followed upon diphtheritic disease of the throat, and in which the danger of destruction of the internal ear, and perhaps life-long disease of the organ, if not early death, were entirely prevented by early examination and prompt paracentesis of the membrana tympani, followed by application of cleansing solutions of boracic acid. Great relief was at once afforded to the ear-ache and vertigo, from which the patients had previously suffered, and a cure resulted in five and seven weeks in the two cases respectively. (*Berlin klinische Wochenschrift*, 43, 1882.)

Diabetes Insipidus Treated with Ergot.—Dr. Carter of Birmingham gives the details of a case taken to be one of diabetes insipidus in which ergot was used without success. The patient was a girl of six, well up to twelve months before admission, at which time thirst, diuresis, and emaciation set in. She never had any other complaint except measles. On admission she weighed less than 28 lbs.: appetite good, not ravenous; thirst intense; bowels constipated. Urine limpid, neutral, sp. gr. 1007, without albumen or sugar, daily average 76 oz. She began with 15 minims of liquid extract of ergot three times a day, and this was gradually increased to a drachm and even a drachm and a-half given *quartis horis;* but the variations in the quantity of urine seemed to have no relation to the administration of the drug. Quinine was ultimately given, and under this the general condition improved, but the special trouble was unaffected. On discharge, after four months' treatment, the patient weighed 32¼ lbs., and passed 102 oz. of water daily. Dr. Carter regards the ergot as inoperative on the following grounds: (1) the flow of urine increased for the first three weeks, though ergot was taken; (2) ergot was resumed in half-drachm doses after suspension for a fortnight, yet the average daily flow for the next four days was 16 oz. greater than during the suspension; (3) on the final suspension of the ergot the daily average diminished at first, and did not afterwards exceed the flow during the time that nine drachms of ergot were being taken daily; (4) such improvement as did occur seemed more closely related to the improvement of general nutrition than to anything else. [Compare *Practitioner*, xxix. 209.] (*Birmingham Med. Rev.*, Dec. 1882.)

Case of so-called Imperforate Hymen.—At a recent meeting of the Obstetrical Society of London, Dr. Matthews Duncan read a paper with this title. He was induced to relate

the case by three circumstances. (1) There was a remarkable absence of any kind of suffering during nearly the whole time of the development of the disease. The patient had never menstruated nor suffered from any uneasiness in connection with that function until eight months before admission, when she was told by a medical man that she had a lump in the lower belly. Since then she had suffered from irregular achings. The author thought the probable explanation of this was that the uterine body was not distended, for facts showed that dilation of the uterine body was more difficult and painful than dilation of the vagina and uterine cervix. (2) The case illustrated the treatment without any injections which had been the subject of remarks at a recent meeting of the Society. An incision was made by Paquelin's cautery knife. About twenty-five ounces of the usual treacly fluid escaped, about twenty ounces on the following day, and the last of it on the fifth day, in all about fifty ounces. At no time had it any fœtor. No hypogastric pressure or interference with the flow was permitted. A piece of carbolised lint was put to the vulva. The patient made an uninterrupted recovery. He thought the risk of peritonitis was increased by the washing out sometimes practised. He thought the cautery knife was preferable to any other means of making the incision, because its wound was not an absorbing surface. (3) The condition of the pudendum rendered the term "imperforate hymen" erroneous and misleading. The vagina was closed by a membrane upon which the hymen could be seen entire and healthy, and after the operation the hymen could be seen to have its normal position and relations. He had made the same observation in other cases, and he had seen the hymen present when vagina and uterus were both absent, On these grounds he regarded the view of M. Budin (that the hymen was nothing but the anterior extremity of the vagina) as incorrect. (*Lancet*, Oct. 28, 1882.)

Extracts from British and Foreign Journals.

Results of Occlusion of the Renal Artery.—Dr. Joseph Werra has made a number of experiments on this subject in the Pathological Institute of Professor Langhans. Ligature of the renal artery for one hour causes alterations in the kidney which affect chiefly the epithelium of the cortical and to a less extent that of the medullary substance. The descending portions of Henle's loops are not affected at all. The cells become turbid and a few desquamate; the nucleus loses its power of absorbing colouring matters, and calcification occurs. This becomes persistent when the ligature is applied for two hours, and the epithelial cells must then be regarded as dead; but when the ligature is applied for one hour only, the lime salts are absorbed in the course of two or three weeks, and the normal structure of the tubules is again restored. From experiments made by the injection of sulph-indigotate of sodium, it would appear, although this is not quite certain, that the function of the kidney becomes completely restored, as well as its anatomical structure, after ligature of the artery for only one hour. Permanent obstruction of the renal artery produces at once a great enlargement of the organ, which at first is twice or three times as large as normal. About the third day it gradually begins to contract, and finally becomes smaller than normal. At first it is congested, but from the second day onwards it is anæmic, with hæmorrhagic infarctions. There are two parts of the kidney which are not deprived of blood, and do not seem greatly injured by the ligature; these are the layer of the cortex immediately under the capsule, and the border-layer between the cortex and the medulla. The former evidently receives blood by collateral circulation from the vessels on the surface; and the latter from the vessels of the hilum. On microscopic examination, the contorted tubuli, the ascending limb of the loops, and the border-layer, all show similar appearances. In the first ten or twelve hours the epithelium becomes turbid, the nuclei are less readily stained, and the lumen is filled and generally completely obstructed by a some-

what glistening granular mass. From the third day onwards, the nuclei hardly stain at all. In the first few days the epithelial cells desquamate, and with the granular mass already described form granular cylinders. From the third day on, some of the tubules undergo calcification. There are two places where this chiefly takes place: viz. one upon the surface of the kidney, closely though not immediately under the capsule; and a second which reaches nearly to the base of the pyramid. The cause of this is readily explained: it is at this point that the collateral circulation becomes established, but only at a time when the epithelium is already so altered that a deposit of lime takes place in it. In the collecting tubules the same alterations occur as after temporary ligature, viz. (*a*) contraction, and (*b*) a granular condition of the epithelium, with desquamation of both the contracted and granular cells. Granular masses and threads of fibrin often appear in these tubules. The descending tubules of the border-layer and papilla are least altered of all. The nuclei always remain visible. The lumen at first generally contains red corpuscles, and later on, a granular mass. The connective tissue is much damaged. The glomerular loops become indistinct, and there is generally a granular mass with red corpuscles and threads of fibrin in the lumen of Bowman's capsules, which sometimes greatly compress the vascular loops. The vessels are little altered. At first there is hyperæmia, and later on, anæmia. The hyperæmia is due to the flow of blood into the kidney from the collateral vessels, while the nutrition of its own blood-vessels is so altered that they yield before the pressure. The anæmia appears to be due to an alteration in the blood which is stagnant in the vessels; it becomes converted into a granular mass, and is absorbed. The stroma is at first little altered with the exception of some infiltration. From the third day on, it is constantly infiltrated with lymphoid corpuscles. These chiefly come from the vessels between the cortical and medullary substance, and also from those of the capsule. The stroma is at first increased by the infiltration, afterwards it diminishes on account of the contraction of the connective tissue. By this process the tubules are compressed. The result of permanent ligature is therefore atrophy, caused by interstitial inflammation which starts from the points where the collateral vessels anastomose under the capsule on the border between the medulla and cortex. It is remarkable that the papillæ remain free from inflammation. (*Virchow's Archiv*, Vol. 88.)

The Cure of Lichen Ruber without Arsenic.—Dr. Unna states that he has found *lichen ruber exudativus* curable by purely external measures. Out of thirteen cases of the disease six

were completely cured in from eight days to three weeks by the following ointment:—

℞ Ung. zinci benzoat.	500·0	grammes
Acid. carbolic.	20·0	„
Hydrarg. bichlor.	0·5	„

The bichloride is sometimes increased to twice the above strength. (*Monatsch. f. prakt. Dermatol.*, quoted in *Journal of Cutaneous and Venereal Diseases*, No. 2.)

Treatment of Soft Chancres and Buboes by Salicylic Acid.—Autier states that salicylic acid is highly efficacious as an application to soft chancres and buboes. It is odourless, only slightly painful in its application, soluble in alcohol and glycerine, and leaves no stain on linen: for these reasons it is preferable to some other agents commonly used. (*Thèse de Paris*, 1881, quoted in *Journal of Cutaneous Diseases*, No. 2.)

Gonorrhœal Rheumatism.—Dr. W. Nolen terminates an elaborate article on gonorrhœal rheumatism somewhat as follows: While occasionally gonorrhœa is complicated by a rheumatic affection, such cases are very rare in proportion to the immense number of cases of gonorrhœa not so complicated, and even when present, the rheumatism may be of the most diverse nature and intensity, and possess no distinctly characteristic features. It seems to be fairly well established that when once an attack of gonorrhœa is complicated by rheumatism, it is also present in subsequent attacks, indicating the probability, in some unknown manner, of the causative action of the gonorrhœa; though it may also be held that the principal cause lies not in the gonorrhœa, but in the constitutional tendencies of the individual, the simultaneous occurrence of gonorrhœa and rheumatism being merely a coincidence. (*Deutsches Arch. f. klin. Med.*, Nov. 1882.)

Embolism of the Hepatic Veins.—Emboli coming from the systemic vessels are occasionally found in the hepatic veins. The usual explanation is that they are due to a brief reversal of the blood-current in the inferior vena cava or larger hepatic trunks. Ewald has made some experiments on the subject and has made it probable that the true explanation is different. He shows that an embolus may descend from the superior vena cava or from the thoracic part of the inferior vena cava by its own weight against the blood-current, and so may reach the hepatic veins. The condition for this is that the patient should maintain the erect position (sitting or standing), and that the embolus should be formed somewhere in the territory of the superior vena cava. The whole paper is worth consulting. (*Berl. klin. Woch.* 33, 1882.)

Simple form of Cautery.—Dr. Post says (*Phil. Med. Reporter*, Sept. 23, 1882) that he is in the habit of employing a form of actual cautery which, upon the whole, he much prefers to Paquelin's cautery. It consists of six short wires grouped together, with which, after heating it in a lamp close at hand, six small burns can be made simultaneously, instead of one after another. If heated only moderately, a number of small scars can be made, which for many purposes is preferable to the single eschar produced by a large cautery. The amount of irritation produced by this multiple cautery is very moderate, and the burning sensation resulting from its application is relieved as by a charm by a strong solution of bicarbonate of soda. After the first day an ointment made of drachm of extract of stramonium to an ounce of vaseline greatly relieves the pain. Dr. Post finds this cautery very useful in almost all cases of chronic inflammation about joints, especially when it is attended with great induration of surrounding tissues, and also in various instances of deep-seated pains. (*Med. Times and Gazette*, Nov. 11, 1882.)

Bacilli in Croupous Pneumonia—Griffini and Cambria have been working at the question of the etiology of croupous pneumonia, and the following is an abstract of the results of their investigations. There is, they conclude, a bacillus in the blood and sputa of pneumonic patients, which is not the same as Klebs' *monadine*. When the pneumonic sputum, free from saliva, was subcutaneously injected, or placed in contact with the trachea of rabbits and dogs, a fatal form of septicæmia was induced, and the blood of these dead animals when inoculated into others also led to a fatal result. The authors maintain that this is not a specific effect of pneumonic sputa, for some old experiments by Senator have shown that the inoculation of fresh bronchial mucus under the skin of a dog was followed by a deadly result. The saliva of pneumonic patients kills rabbits rapidly, but only gives rise to an abscess at the site of injection in dogs. Defibrinated blood from cases of pneumonia, thrown into the cavity of the peritoneum or of the trachea of rabbits and dogs, resulted not in pneumonia, but in various degrees of fever. Inoculations of the bacilli in various stages of artificial cultivation never caused any changes in organs, but only alterations in temperature. The bacillus of the pneumonic blood and sputa was always afterwards found in the blood of the vaccinated animals, although it had excited no pneumonia; hence, the authors believe that the bacillus found in the patients suffering from pneumonia was an epiphenomenon, and not a necessary factor in the production of pneumonia. When small doses of ammonia were injected into the trachea of animals, lobar

pneumonia was invariably produced if the animals survived a few days. The authors come to the conclusion that pneumonia is not an infective disease. (*Centralblatt für klinische Medizin*, No. 25, 1882.)

The Preparations of Aconite.—Dr. Squibb, in his "Ephemeris," states that the fluid extract of the root is the best preparation for internal use. The dose is one minim every three hours, or oftener. For external use there is probably no better form than an oleate of aconitia, made by dissolving two grains of aconitia in ninety-eight grains of oleic acid. A fluid ounce of oleic acid, weighing 412 grains, requires 8·25 grains of aconitia to make a 2 per cent. solution, each minim of which contains about one-sixtieth of a grain. This quantity, applied locally, is a sufficient dose, and should in a short time produce constitutional effects by absorption. It should be applied to the surface by the cork of the phial or by some non-absorbent, and about the head and face needs no covering, great care being taken that it does not get into the eyes. If applied under the clothing it should be covered with oiled silk or rubber tissue. Local neuralgias are much better reached by the dermic or epidermic method. (*New York Med. Record*, Nov. 25, 1882.)

Vomiting in Phthisis.—In order to relieve this symptom of such frequent occurrence in phthisis, Dr. Woillez painted the pharynx with a solution of bromide of potassium, and found it very useful. A pencil of charpie dipped into a solution of pure bromide in two-thirds of water was passed rapidly into the pharynx before meals, the patient being required to abtain from expectoration afterwards as long as possible. In several cases the vomiting was arrested by the first application, while in others the action, though less immediate, was also beneficial. Fifty-two applications having been made in nine patients, vomiting only occurred seven times, the operation always being performed before any food was taken. (*Jour. de Thérap.* Oct. 28, 1882.)

Diagnosis of Ecthyma.—Dr. McCall Anderson in a lecture on Diagnosis of Diseases of the Skin has these remarks on ecthyma: Ecthymatous pustules and crusts are apt to appear in non-syphilitic subjects, especially in those whose health is deteriorated by bad diet, intemperance, etc., and who are uncleanly, or who are affected by scabies or other itchy eruptions, and may be mistaken for syphilitic ecthyma. The following points are of service in the diagnosis:—

Ecthyma Cachecticum.	*Ecthyma Syphiliticum.*
1. Commonest on lower extremities and hips—never on face.	1. May affect any part, and not uncommon on face.
2. Ulcers not so prominent a feature, and their character depends on the general state of patient.	2. Ulceration characteristic of Syphilis.

3. Surrounded by a reddish areola.	3. Surrounded by a coppery areola.
4. The crusts brownish and rarely rupiform.	4. The crusts greenish or blackish, thick, rough, very adherent, and often rupiform.
5. Cicatrices present no special features.	5. Cicatrices coppery, or white with coppery edges.
6. Occurs in broken-down subjects, or in those affected with some itchy eruption, such as Scabies.	6. Occurs in persons tainted with Syphilis, and usually the subjects of other syphilitic manifestations.
7. Heals readily under simple local treatment and attention to the general health.	7. Cured by anti-syphilitic remedies.

(*Med. Times and Gaz.*, Nov. 25, 1882.)

Peri-splenic Abscesses.—In an article on purulent peri-splenic collections, C. Zuber reports two interesting cases in which such purulent collections were found after death, the true character of which had not been suspected during life. In one of the cases the post-mortem revelation was a complete surprise, because no symptoms had been observed calling attention to the left side ; and in the other there was an error of diagnosis, as the case had been considered as one of intra-abdominal malignant disease. The notes of these cases are given at length ; both had a history of repeated malarial attacks. Following these is a brief consideration of other cases contained in medical literature of this very rare form of disease. The more important points of this essay may be summed up as follows :—(1) In the upper portion of the abdomen are found purulent collections, which are called peri-splenic abscesses, although they only touch the spleen at one part of its surface, and are not at all localised in the sub-serous connective tissue of the spleen. They occupy by preference the irregular space bounded by the stomach, the spleen, the colon, and the diaphragm. These collections are the last stage of circumscribed peritonitis, due ordinarily to lesions of the spleen or the digestive tube. The infectious form of splenitis (comprising herein the lesions of malaria), and the round ulcer of the stomach, appear to play the principal rôle in these intra-abdominal abscesses. (2) The purulent collections of digestive origin contain gas, and their character is shown, with remarkable uniformity, by a resemblance more or less complete with pyo-pneumothorax, the more so because they are only separated from the pleura by the diaphragm, which is strongly pushed upward. The nature of these cases of false pyo-pneumothorax will be recognised at first by the existence of grave digestive disorders, and subsequently by the variability, the exaggeration or insufficiency, of the symptoms observed. The collections of splenic origin are scarcely ever characterised by tumefaction and pain of the hepatic region and the general signs of latent suppuration; rarely, by tumours more or less marked or fluctuating. The diagnosis will scarcely be made

except by exclusion. (3) Whatever may be the origin, the depth, or the extent of these peri-splenic collections, they are not above the resources of modern surgery. It is this practical point of view which will dominate the question. No effort should be spared in order to determine the existence, and then the nature, of the abscess; and exploratory punctures, either deep or multiple, should not be too much feared. Made methodically and prudently, such explorations bring only an insignificant danger, as the recent literature of hepatic abscess abundantly shows; they alone may be, on the contrary, the point of departure of a truly rational and useful therapeutic method. (*Revue de Médecine*, No. 11, 1882.)

Results of Sponge-Grafting.—Dr. Estes has used Hamilton's method in seven cases of deep indolent ulcer with excellent results. He makes a comparison of the two methods of sponge-grafting and skin-grafting in such cases which he sums up in the following conclusions:—(1) Sponge-grafts are available when skin cannot be obtained. They cause no pain in preparing them, nor any annoying little wounds as an additional tax on the healing powers of the patient. They do not subject the recipient patient to the danger of inoculation of specific diseases—as skin may do when taken from a cachectic donor. (2) Sponge-grafts take more surely; invariably, when proper care is exercised. (3) Sponge-grafts stimulate marginal activity in the epidermis much earlier and to a much greater degree than skin. (4) In sponge-grafting skin or cicatricial islets are much slower of formation, and not as sure as after skin-grafting. (5) Healing seems equally rapid, if not more rapid with sponge-grafts than with skin. (6) Resulting cicatrices are equally good and contractions equally prevented. [*Practitioner*, xxviii. 294.] (*Philad. Med. News*, Nov. 25, 1882.)

Healing of Cautery-Wounds.—It is usually supposed that healing by first intention is not to be looked for in wounds produced by the actual cautery. Observations of MM. Nicaise and Fort would show, however, that immediate reunion is quite possible. Two conditions are necessary. The film of burnt tissue must be thin, in order that the new vascular loops may be able to penetrate it. The film will be thin if the cautery be thoroughly hot and the tissue not very resistant. Thus the eschar of cauterised skin will be thicker and tougher than that of muscle or cellular tissue. The second condition is that the wound is kept perfectly aseptic. (*Bull. de la Soc. Chir. Paris*, vol. viii.)

Pathogeny of Varicose Ulcers.—With regard to varicose ulcers one thing is admitted—that they are ultimately a result

of varicosities of the veins. But there must be some further determining cause, for many persons have varicose veins without ulcerations. The other condition we take to be a neuritis of the cutaneous filaments. This we might suspect *à priori* from Terrier's experiments on the disordered sensibility to heat which is noticeable near the parts about to ulcerate, and from the other signs of disturbed nutrition which accompany the ulcer—*e.g.* desquamation, abnormal pigmentation, changes in the hair, pemphigoid eruptions, &c. These disorders have struck many observers, and particularly Verneuil and Reclus. Moreover, the œdema which usually appears early in the course of the affection we might refer, with Ranvier, to nervous disorder. Quénu has confirmed these presumptions by his recent histological researches. He finds that in every case of varicose ulceration the nerves of the limb affected show signs of interstitial neuritis between and around the fibrils: the morbid change starting in the venules belonging to the nerves. The neuritis is not merely due to the involvement of the nerves in the ordinary inflammation surrounding the ulcer, for it is found in portions of the nerve-trunk far removed from the latter. It is not an ascending neuritis beginning in the filaments round the ulcer. If it were, the morbid change would be confined to the nerves whose distribution corresponds to the region of the ulcer; but this is not the case. The neuritis is in fact a primary affection of the nerve; it corresponds either to the course of the larger diseased veins or to that of the dilated venules supplying the nerve. This varicose dilatation speedily issues in periphlebitis, and this extends to the fibrous sheath of the nerve itself. Rest is the true cure for varicose ulcerations, as it is the best for perforating ulcer of the foot: this latter is beyond doubt trophoneurotic in its origin. (*Rev. de Chirurgie,* Nov. 1882.)

The Galvanic Current as a Cardiac Stimulant.—Professor Von Ziemssen claims that the ordinary faradic electric applications completely fail to alter the action of the heart or to disturb in any way its sensibility, while, on the contrary the direct or battery current produces a powerful stimulating effect. The importance of this observation in the treatment of narcotic coma and syncope is very great. The result is the more remarkable as the experiments to support it were made upon the human subject, a woman having lost a portion of the anterior wall of the chest by an operation for the removal of an ecchondroma, so that the heart was partially exposed. (*Deutsch. Archiv. f. klin. Med.*, No. 30, 1882.)

Sulphate of Atropia in Earache.—Dr. A. D. Williams recommends (*American Chemists' and Druggists' Bulletin*) the

local application of a solution of sulphate of atropia of a strength varying with the child's age. Under three years, one grain to ℥j, over ten years, four grains to ℥j. It may be used warm, and is to be dropped into the ear and allowed to remain ten to fifteen minutes. In grown persons almost any strength may be used. It is practically a specific in the recurring nocturnal earaches of children, but only a slight palliative in acute inflammation of middle ear, and in acute inflammation of the external meatus. (*British Medical Journal*, Oct. 28, 1882.)

Perforation in Enteric Fever.—Dr. Byers, in a paper read at the annual meeting of the British Medical Association at Worcester, calls attention to the great frequency of perforation in typhoid fever, it being found on the average in one fifth of the fatal cases. He has found that this may be dreaded in all cases in which there are any signs of severe or deep ulceration of the bowel; such are, the type of the disease being unusually severe; extreme tympanites or constipation, a single deep ulcer paralysing the action of the bowel and allowing an enormous accumulation of flatus, or causing constipation; continued elevation of temperature after the third week; severe tremor; protracted headache in the early stages; and lastly, the persistence of the *tâche cérébrale* during convalescence. A case terminating fatally from perforation is cited, illustrating most of these points. From the great fatality of this complication, Dr. Byers strongly urges, when one or more of the above symptoms are present, that the following precautionary measures should be adopted. The patient is to be kept perfectly quiet in the dorsal recumbent position. All food is to be given in the liquid form and purgatives are not on any account to be administered, and opium must be given regularly; this places the ulcers in a better condition for healing, and the chance of rupture of their floors, from sudden movements of the intestines, is minimised. (*British Med. Journal*, Nov. 4, 1882.)

Quinine and Potassium Chlorate in Malaria.—Dr. Baldini (of Grado, west coast of Africa) reports that he has in over seventy cases of intermittent fever cut short the attack as with one blow by using these drugs in combination. He gives for a dose twenty to thirty grains of sulphate of quinine, with fifteen grains of potassium chlorate in a number of pills. For children he suspends the powders in milk. (*Med. chir. Rundschau*, Dec. 1882.)

Trichlorophenol as a Disinfectant.—Prof. Dianin prepares this substitution product ($C_6H_2Cl_3OH$) by acting on carbolic acid (phenol) with chloride of lime. Extensive clinical and other experience lead him to the following conclusions

regarding it. (1) It is twenty-five times more powerful as a disinfectant than carbolic acid. (2) The very smallest quantity of it suffices to check fermentive action of any kind. (3) It is therefore an invaluable antiseptic exceeding in efficiency all the other substances usually employed (such as permanganates, chlorides, carbolic, boracic and salicylic acids, and thymol. (4) It deodorises as well as disinfects: its own smell is removed by oil of lavender. (5) The solid substance is very slightly irritating to the tissues, the solution not at all. (6) It has proved unmistakably useful as a local application in soft chancre, diphtheria, &c. (7) It can be prepared by the physician for his own use. (8) Its salts have similar properties to its own: the sodium salt is inodorous. (9) The calcium salt is cheaper than carbolic acid itself. (*St. Petersburg med. Woch.* 38, 1882.)

Bacteria in Gonorrhœa.—At the Berlin Charité Medical Society, Dr. Leistikow read a paper, which is reported in the *Berliner Wochenschrift*, No. 32, detailing the results of his researches on gonorrhœal bacteria, first discovered by Neisser. They are found in the pus-cells, this locality distinguishing them from other bacteria. They occur *only* in the gonorrhœal affections of the urethra and conjunctiva, and are never found in urethral or conjunctival secretions arising from other causes. In the early inflammatory stage they were found proportionately in smaller numbers than in the gleety stage, when they were always present. Inoculations of gonorrhœal pus from the urethra and conjunctiva have hitherto given only negative results. The various remedies used in gonorrhœa, such as zinc, lead, tannic or carbolic acids, after a few injections dispersed all the bacteria, as did the copaiba balsam when taken internally. Corrosive sublimate also prevented the development of bacteria, and Dr. Leistikow especially recommends it as the best means of treatment of gonorrhœa, employed in an exceedingly weak solution (10 to 20,000 or 30,000). With any of these injections we should persevere for some days after disappearance of bacteria, or they will return again. (*Petersb. med. Woch.*, Oct. 28, 1882.)

Notes and Queries.

NOTE TO DR. HOBSON'S ARTICLE ON PERNICIOUS ANÆMIA, p. 24.—It has been suggested to me that the absence of references to my cases renders them useless for statistical purposes, because it would be impossible to avoid counting cases twice over if they were included in larger lists. I intended at one time to put my cases in a tabular form in which the references would be inserted, but I knew that tables were troublesome to print and troublesome to get into the mind when printed, and so I put them into the form in which they appear in the appendix to my article. I now give the references under heads of the different publications in which the cases appeared.

British Medical Journal.—Dr. King: case 28, 1871, vol. ii. Dr. Finny: cases 8, 21, 22, 1880, vol i. Dr. W. G. Smith: case 33 (Dublin Path. Soc.), 1880, vol. i. Dr. Broadbent: case 3, 1880, vol. ii. Dr. Coupland: cases 9, 11, 14, 15, 16, 26, 29, 1881, vol. i. Dr. Clifford Allbutt: case 4, 1881, vol. ii. Dr. Withers Moore: case 41, 1881 (also by correspondence with Dr. W. Moore).

Lancet.—Dr. S. Mackenzie: case 2, 1878, vol. i. cases 10, 30, 43, 1878, vol. ii.

Medical Times.—Dr. Habershon: case 31, 1863, vol. i.; case 13, 1876, vol. i. Dr. Andrew: case 1, 1877, vol. i. Dr. Byrom Bramwell: case 42, 1877, vol. ii.

Guy's Hospital Reports, 1878.—Dr. F. Taylor: cases 5, 6, 7, 17, 18, 19, 20, 23, 24, 25, 27, 32, 34, 35, 36, 39, 40, 45.

Practitioner.—Dr. L. H. Jones: case 38, vol. xxiv. Dr. Hobson: case 37, vol. xxx.

Pathological Transactions.—Dr. Dickenson: case 44, 1862. Another case of Dr. Dickenson's: 12, was mentioned in a somewhat recent discussion, I think, at one of the societies, but the exact reference I cannot trace. Some of the facts I obtained by correspondence with Dr. Dickenson.

Umney's Fluid Extract of Cinchona.—This extract is made from the Indian red bark. The culture of the red bark in India has of late years become very important, and the quality of bark produced is not only equal to, but in many cases surpasses the best yellow bark.

The present preparation appears to us to be quite equal to the ordinary preparations from the yellow bark. It is by no means easy to analyse such preparations, and to get from them trustworthy results. We have had this analysed with the following results, which we think, considering the difficulties of correctly estimating the amount of alkaloids in galenical preparations, bears out the opinion we have already expressed :—

	Grains in the Fluid Ounce.
Quinine	1·37
Cinchonidine	1·71
Cinchonine	0·68
Amorphous alkaloid	4·27
Total alkaloid	8·03

Most of the alkaloid is probably present in the state of tannate, or rather cincho-tannate. One fluid ounce of the liquid extract contains 220·92 grains of extract dried at 212°.

Benger's Digestive Ferments.—We have received from Messrs. Mottershead and Co. four preparations :—(1) *Liquor Pancreaticus* (Benger); (2) *Liquor Pepticus* (Benger); (3) *Benger's Peptonised Beef Jelly;* (4) *Benger's Self Digestive Food.* The first two contain only the digestive principles, the latter contain nutriment in addition. Since the publication of Dr. William Roberts's remarkable lectures on the use of ferments, they have come more than ever into general employment in cases of dyspepsia and mal-nutrition. Both the *Liquor Pepticus* and the *Liquor Pancreaticus* (Benger) are preparations of great activity, and possessing, as they do, to such a great extent, the digestive powers of the natural juices, they are very useful indeed when the latter are deficient. The *Liquor Pancreaticus* especially we have found useful in cases of indigestion and acidity coming on about two or three hours after meals; a dose of this administered with a little bi-carbonate of soda at that time, seemed to restore comfort, and enabled the digestive process to be carried on to its natural fermentation.

The Peptonised Beef Jelly is a solid beef-tea which differs from ordinary extracts of meat in containing much of the fibrin in a partially digested and soluble condition.

The Self Digestive Food is a farinaceous flour pancreatised,

so that when mixed with warm milk not only the farinaceous matter, but the proteids of the milk become digested. The rapid digestion thus ensured enables the food to be more easily absorbed, and prevents the decomposition, with formation of irritating products, which might otherwise occur in the stomach. It can therefore be retained by persons suffering from dyspepsia or irritable stomach, and children when other foods are rejected.

All these preparations are, we think, deserving of the highest praise, and only require to be made more generally known to the profession to insure their extensive employment.

First Swiss Alpine Milk Company.—We have received from the First Swiss Alpine Milk Company specimens of preserved and condensed milk. We believe them both to be very good, and we have been favoured with the opinion of a celebrated physician which confirms our own. Both preparations seem to be genuine and reliable.

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*** Any of the foreign works may be procured on application to Messrs. DULAU, of Soho Square, W.C.; WILLIAMS and NORGATE, of Henrietta Street, Covent Garden, W.C.; or BAILLIÈRE, of King William Street, Charing Cross.

Department of Public Health.

PUBLIC HEALTH IN MICHIGAN.

In the preparation of their Annual Reports, many of the State Boards of Health in the United States seek not only to supply information concerning the health of the state which is under their immediate supervision, and to bring together matter which shall be of use to their own and other health officers, but also to enlist public sympathy with their work by the reproduction of papers and lectures bearing upon public health questions, and which have an interest for every educated citizen who desires to promote the public welfare. The ninth Annual Report of the State Board of Health of the State of Michigan is a conspicuous example of this, and amongst the papers which have been embodied in it by the Secretary, Dr. Henry B. Baker, there is one having a direct bearing on this subject. Under the title "Personal Sanitary Responsibilities," Mr. John K. Allen of Lansing deals with the responsibility of individuals to the state, to other persons and to themselves, in regard to public health.

The power of the people in state, as in other, affairs, depends essentially upon their health. The prosperity of a state is to a considerable extent to be measured by the number of working days enjoyed by its citizens; this again depends upon the health of the individual, and hence it becomes the duty of every subject loyal to the state to do all in his power to place every citizen under the most healthful conditions procurable. Many fancy their first duty as to this is to join in the cry for additional legislation, but the writer of the paper urges rather that much may in the first instance be done by the proper enforcement of the existing laws. The law, he alleges, gives

such power to boards of health as would enable them to throw a mantle of comparative safety around every citizen and his family, and it is a part of the duty of the citizen to see that the board enforces its laws in this respect. If the board fails properly to fulfil the position of trust in which it has been placed, then it becomes a duty devolving on every townsman to cast his votes for men who will be more faithful. The lesson here taught as to the right attitude of the individual towards the health authority is one as much required on this side of the Atlantic as on the other.

Dealing next with the responsibility of the individual to other persons, Mr. Allen bases his remarks on a statement once made by Lord Derby to the effect that no sanitary improvement worth the name will be effected unless a real and intelligent interest can be created with regard to it among the people at large. A movement of sanitary reform, to be successful, must be embraced by the people as a whole, and everything should be done both by education, precept and example, to secure their intelligent co-operation. Every one can to some extent aid in this, and numerous instances are given to show how the individual householder may, by securing the highest procurable conditions of healthiness about his own premises, influence his neighbours for good.

In regard to what are our sanitary duties to ourselves, it may be said that in so far as we seek to improve our own health and to surround ourselves by healthy circumstances, we do an indirect good to the state, and by force of example we influence our fellow-citizens. So also, much more may often be done in this way than by attending health conferences and conventions, whilst neglecting in our own immediate surroundings those conditions of personal health and cleanliness which are necessary if we would maintain at a high standard of perfection, not only our bodies, but also our minds.

ALLEGED HINDRANCES TO ISOLATION.

Amongst the causes which have led sanitary authorities to neglect the provision of means for the isolation of infectious diseases in their districts until it has been forced upon them by the actual presence of some epidemic disease, there are several concerning the operation of which much useful information may be gathered from Dr. Thorne-Thorne's recently issued official report on the Use and Influence of Hospitals for Infectious Diseases.[1] Some of these causes we propose to advert to in detail, in the hope that authorities who may still be deferring the provision of one of the principal means of defence against the spread of infection, may learn from the experience of other districts how rarely the erection of an isolation-hospital during the course of an epidemic is attended with any success in dealing with the actual emergency which has led to its provision.

1. *Temporary Hospitals.*—The conviction that some temporary hospital can be hurriedly erected whenever the need for means of isolation arises, has been so frequently expressed, that we may give a foremost place to this reason for delay. Besides which, nothing could be much more striking than the proof that has been afforded to the effect that the result of acting on any such belief is injurious in the extreme, both as regards the immediate and the permanent sanitary interests of the districts concerned. Unfortunately a by no means inconsiderable number of infectious hospitals have been provided under the influence of a panic resulting from the actual prevalence of disease. Turning to their history, we find that much greater difficulty than was anticipated has, in most cases, been met with in securing a site; indeed in many places that difficulty has been such as to prevent, for the moment, the erection of any hospital at all. But, even where it has been overcome, the delay has been serious. The actual erection of the buildings has also taken a longer time than had been hoped for, and by the time that sufficient furniture and fittings have been provided to allow of the admission of patients, it has, in an important proportion of instances, been found either that the epidemic had entirely abated, or that the few remaining

[1] Supplement to the *Tenth Annual Report of the Local Government Board*, by the Board's Medical Officer (c. 3290). 1882.

patients were either too ill to be removed from their homes, or so far convalescent as to render isolation all but unnecessary. But even where they have been of some use in connection with the actual prevalence of disease which has led to their provision, their hurried erection has had a serious influence on their after history. The buildings, consisting as they have done, either of wood, or more rarely of iron, have in most cases resembled barns or sheds rather than hospitals, and they have been so deterrent as regards their exterior, that patients who would otherwise willingly have submitted to removal from their homes for the purposes of isolation, have very generally refused to enter them.

The materials too, imperfect in themselves, and hurriedly put together without proper supervision, have soon shown signs of their defective character, and repairs have become necessary in order to keep out wind and rain, to say nothing of what has become necessary in order to maintain them in a state of fitness for the reception of patients; and thus it has come to pass that buildings over the construction of which large sums have been expended, may be found empty and decaying in various parts of the kingdom, whilst their very existence is alleged as an excuse for making no further isolation-provision. It is true that some of these ugly makeshifts have been of certain use for the isolation of cases of small-pox, but the circumstance that they have served their intended purpose in connection with this disease, is not to be regarded as a proof that they will be found equally available for the isolation of the other infectious fevers. But little difficulty is as a rule experienced outside the metropolis in securing the removal of small-pox patients to hospital, but the parents and friends of patients have, in the presence of scarlet-fever for example, none of that feeling of horror with which the more loathsome disease, small-pox, is associated in their minds, and hence it comes to pass that authorities who, in the face of an outbreak of small-pox, have made some sort of hospital provision, have experienced little or no difficulty in securing the isolation of that disease during a series of several years, whereas they have, owing to the nature of their temporary building, never succeeded in using it for any other disease. This circumstance is the more to be regretted, because small-pox is certainly not the infectious disease which

most loudly calls for the provision of means of isolation. Indeed from at least one point of view it may be looked upon as the one which least calls for any such measure of prevention, for it is the only one of the infectious fevers against attack from which the public are enabled efficiently to protect themselves; and this at the public cost. So also, since the more general enforcement of vaccination, the mortality from small-pox has fallen far below that of several of the other infectious fevers. Thus, whereas the total deaths from small-pox registered in England and Wales during the ten years 1870-79 were 57,433, those from scarlet fever during the same period reached 173,724. Still, small-pox far outweighs all the other infectious fevers in the extent to which it has secured the erection of isolation-hospitals; and in by far the majority of instances the buildings which have been put up in order to prevent its spread have turned out to be all but useless so far as the isolation of infectious diseases generally is concerned.

Reading through the details given in the official report as to hospitals which have been the outcome of panic resulting from a small-pox outbreak, and having regard, amongst other things, to their cost and to their after-history, we have no hesitation in saying that, except under very special circumstances, it is all but useless for sanitary authorities to entertain the provision of temporary hospital accommodation when small-pox has already begun to spread in their districts. Vaccination being relied upon for the immediate emergency, the occasion should be turned to account for commencing the erection of some small permanent hospital, which will, in the case of any future importation of that or any other infectious disease, suffice for the immediate isolation of the first patient or patients attacked. If a site be available, and the weather be suitable, tent-hospitals may under the circumstances referred to be provided, and they have this additional advantage that their provision has rarely stood in the way of the erection of a permanent hospital. It must, however, never be forgotten that both the immediate and the permanent usefulness of a hospital for infectious diseases is invariably impaired by hurried erection; whereas, hospitals which have been erected during non-epidemic periods, when due thought could be given to the conditions of construction best

adapted to the sanitary requirements of the district, have afforded excellent examples of what may be done by way of the prevention of disease, by having an isolation-hospital in actual readiness for the reception of patients.

2. *Isolation of Young Children.*—There are however sanitary authorities who, fully recognising the need for hospital provision in order to stay the ravages of scarlet fever, and admitting that it is this disease which of all others calls for means of isolation, yet allege that since this disease prevails mainly amongst the young, it would be useless, in the face of the known unwillingness of parents to part with their children, to attempt to secure their removal to hospital; and hence nothing is done in the matter. That the difficulty referred to does to some extent actually exist every one will admit, and we are bound to confess that we were hardly prepared to learn the extent to which it has been overcome wherever suitable hospitals have been provided. During the course of Dr. Thorne's inquiry he found a number of hospitals in different parts of the country where the records were so kept as to enable him to ascertain the age of every patient admitted, and having summarised these records he found that out of a total of 4,758 admissions from all causes and at all ages, as many as 2,673 of the patients, or 56 per cent., varied in age from a few months to ten years, the rate varying from 33 to 81 per cent. The hospitals where these results had been obtained were situated in various parts of the kingdom; the populations represented by them had but little in common, some of them inhabiting large manufacturing centres, others agricultural districts; but they were nearly all buildings which, both in point of construction and as regards administration, had acquired a reputation for comfort and excellence which, becoming year by year more and more known amongst the classes for whom isolation is most needed, had led to the success in securing the isolation of children adverted to. The exercise of compulsory powers in effecting the isolation had little or no concern in securing the result, and what is even more striking is the circumstance, that, in a large number of the hospitals referred to, the regulations controlling the admission of visitors to see the sick were exceptionally stringent. In one of the hospitals, namely at Manchester, into which 166 children of ten years of

age and under had been admitted, there is a rule which prohibits all visiting, and so far as scarlet-fever patients are concerned—and these include most of the children admitted—it has only been relaxed in one or two instances, and this in the case of patients whose death was imminent. In others again, visiting is absolutely limited to cases in which grave symptoms have supervened, and as regards a small number only is there anything approaching to leniency of administration in this matter. It must not, however, be supposed that success in the isolation of children will in every case be at once attained. It depends in part on the attractiveness of the building, still more on the reputation it acquires for comfort and excellency of nursing, and on many other allied matters; and in nearly all instances it has been of a progressive character, the disinclination of parents to part with their children often disappearing because of the testimony borne by neighbours as to the comforts which they or their children had experienced during a period of isolation.

There is, however, no question but that authorities may, as a general rule, feel satisfied that when once some suitable means of isolation has been provided, no great difficulties need be anticipated in utilising it for the reception of cases of scarlet fever, and this notwithstanding the fact that the patients who will need isolation will in the main be young children.

3. *Compulsory Removal to Hospital.*—It is by no means uncommon for sanitary authorities and their officers to allege that in view of the insufficient means which the law affords for the compulsory isolation of persons suffering from infectious disease, the provision of a hospital would be practically useless. Under section 124 of the Public Health Act, 1875, it is enacted that "any person who is suffering from any dangerous infectious disorder and is without proper lodging and accommodation," may be removed to an isolation-hospital under a magistrate's order. The question has however arisen whether the improper nature of the accommodation warranting such removal has reference to the patient's own welfare or to that of the community in which he is placed. Having regard to the fact that the Act under which this power is given relates to the health of the public rather than to that of the individual, the latter interpretation might have appeared the more obvious, and

it certainly has been acted on in several instances. So long, however, as any doubt remains upon the subject, some difficulties may be expected to arise from time to time, and it is the anticipation of such difficulties that has led some sanitary authorities to wait for further powers in this matter rather than avail themselves of the powers now at their disposal. But when we come to note the general experience of sanitary authorities who have provided themselves with suitable infectious hospitals, we find, as a matter of fact, that the question of compulsory removal rarely arises. Forming a rough estimate of the total number of patients admitted into the hospitals reported on by Dr. Thorne during the more recent years to which his statistics relate, it would appear that they number some 8,000 to 10,000, and yet we find him saying that one result of his inquiry has been to show how seldom compulsory measures have been resorted to, the cases coming under his notice amounting, in all, to "not more than some three dozen at the outside." Indeed, a reference to the social status of the patients received into the hospitals under consideration, shows that a very large proportion of them came from houses which could hardly be regarded as affording such improper accommodation as is implied in the statute, unless the term were applied with a stringency which we have reason to believe is as yet entirely out of the question in this country. Thus, to quote the experience of a hospital which, notwithstanding an unattractive exterior, has been singularly successful in the isolation of scarlet fever, and which has received an exceptionally large proportion of young children as patients, namely the hospital in the Alcester rural sanitary district, we find that out of 186 non-pauper patients admitted in the three years 1878-80, not one was removed under any compulsory powers; indeed, from the date of the establishment of the hospital in 1875, there had never been any resort to proceedings under the compulsory clause of the Public Health Act.

From this and similar experience elsewhere we may fairly conclude that, with but rare exceptions, it is not necessary to have recourse to compulsion in order to secure the removal from their homes of the infectious sick who need isolation in hospital, and that as regards the exceptional instances, the powers already

conferred on sanitary authorities, if not all that they should be, are by no means so imperfect as is often implied. Indeed, in many districts, opposition to removal, in certain cases where isolation had become urgently necessary in the interests of the public, has been altogether overcome merely by informing the patients and their friends of the existence of the compulsory clause, and in others the procuring of a magistrate's order in one single instance has amply sufficed to remove all trace ot opposition for a long period of years.

In short, the more we examine the ordinary objections to the provision of means of isolation for the infectious sick, the more groundless do they appear, and the more evident does it become that success in this matter may reasonably be expected if sanitary authorities will provide themselves beforehand with a reasonably attractive and well-administered hospital always maintained in readiness for the reception of the first cases of epidemic disease which may arise in, or be imported into, their districts.

THE PRACTITIONER.

MARCH, 1883.

Original Communications.

CLINICAL REMARKS ON THE FUNCTIONAL VOMITING OF HYSTERIA.

BY J. S. BRISTOWE, M.D., F.R.S.

There are few more troublesome affections to deal with than hysterical vomiting, and I suppose that most practitioners of long experience have, at one time or other, been sorely perplexed by cases of the kind. Fortunately, patients seldom die of it, and, even though all kinds of treatment may seem to fail, more or less perfect recovery usually ensues in the long run. I call to mind a few cases which in former years have deeply interested me ; especially two, the one, that of a young unmarried woman, a hospital patient, the other that of a lady of mature years, whose hysterical symptoms were induced in the first instance by severe domestic affliction. The younger patient's vomiting dated from a voyage she had made across the Atlantic some time previously to her admission into the hospital. She continued to vomit after everything she swallowed, and came under my care in a state of great debility and emaciation. She remained in the hospital for some time, suffering from what seemed to be extreme irritability of the stomach, which drugs failed to influence, and which was finally benefited, though not cured, by reducing the

food administered by the mouth to teaspoonfuls of milk only, and by supplementing these by nutrient enemata. The lady presented many symptoms of aggravated hysteria, besides constant and uncontrollable vomiting coming on immediately after everything that was taken into the stomach. She continued in this state for two or three years, became reduced to the last stage of emaciation and helplessness, and on many occasions appeared to be at the point of death; but she recovered. Some years afterwards, having in the interval enjoyed excellent health, she suffered from a recurrence of her malady. In many respects the symptoms were different, but there was a return of the incessant vomiting. She continued in this state for many months, and again she became a living skeleton, and again her life was despaired of. But once more she recovered absolutely; and she remains well. I do not now give these cases in detail, partly because I have said enough about them for my present purpose, but mainly because I hope on some future occasion to devote a special paper to their consideration.

In the spring of last year another case of aggravated hysterical vomiting came into the hospital under my care. The patient was a distinctly hysterical young girl who had been constantly vomiting for about four months, and who had consequently become extremely thin and weak. The abdomen was shrunken, but there was no sign of abdominal disease. Nevertheless, she continued to vomit after admission, exactly as she had vomited before, after everything she swallowed, even if it were only a little water. Various remedies were tried without effect; the food was reduced to milk given in diminishing doses, and ultimately in teaspoonfuls, but still she vomited. Raw meat was then administered, but the result was the same. Then for a week or so nutrient enemata were given to the exclusion of all other food; at the end of which time milk was again tried in minute quantities, and again it was rejected as it had been all along.

The question now for the first time presented itself to my mind, "Was it possible that the girl's vomiting was due, not to irritability of the stomach, but to functional affection of the œsophagus, and that she was being slowly starved simply because no food ever reached the stomach?" There was no doubt

whatever that she swallowed the food. On this point the ward-sister and all who had to deal with her were unanimous. Indeed, I had myself watched her in the act of taking food; and further, I now made her swallow some milk in my presence. The act of deglutition was—it always had been—perfectly performed, the mouthful descended into the œsophagus, and thus got out of the sphere of voluntary action; and then, at the end of a minute or two, after appearing to suffer from a great deal of discomfort she brought it up, as was her custom, without violent straining, but with efforts that fairly well resembled those of vomiting.

The reasons which collectively led me to suspect that her food never reached her stomach were partly personal reasons, and partly reasons derived from experience.

The personal reasons were, that there were never any clear symptoms of indigestion, no uneasiness after food, no flatulent distension or tendency to eructate, and that, so far as I could ascertain, she vomited all kinds of food, liquid or solid equally, no matter how little or how much was taken. It seemed impossible that she could vomit from the stomach, without the most violent efforts, the minute proportions of milk, iced water, and raw beef which were often administered to her, which nevertheless she did reject (after swallowing) almost without change and almost without effort.

The reasons derived from experience were mainly furnished by three cases which presented themselves to my mind. A spare, middle-aged clergyman, of nervous temperament, and liable to megrim, has been in the habit for several years past of consulting me about his ailments, of which the most important has been a peculiar spasmodic affection of the œsophagus (so far as I know quite independent of organic disease of the part), which is apt to attack him at the beginning of a meal, and is usually attended by a painful sense of constriction originating in the lower part of the tube, gasping for breath, and faintness. Now and then, also, he brings up during the night a quantity of mucus, which appears to come from the œsophagus. But the special point in his case, which makes me refer to it now, is that some time since he took at night a dose of morphia for the relief of a threatened attack of megrim, without the expected relief, or even sleep, following, until half

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an hour or so after breakfast next morning, when he became drowsy. He was satisfied that the morphia had lain in his gullet all night, and that it had only been carried into his stomach with his breakfast. His suspicion has since been confirmed; for on several occasions subsequently, his dose of morphia, if it has not been carried on by food taken later, has lain in his œsophagus all night without producing any effect; and either it has been regurgitated in the morning, or its effects have followed his matutinal repast.

The second case was that of a hospital patient of mine, a man over fifty, who had suddenly, about a week before admission, become incapable of swallowing. He had been a healthy man; there was no explanation of his state that I could make out, but he had been wholly without food for a week, and he had consequently become thin, and especially much enfeebled. On making him take food in my presence, I found that he masticated properly, and that the act of deglutition was performed without difficulty, but that immediately what he had taken was violently ejected. The impediment was clearly in the upper part of the œsophagus. Having failed to detect any lesion by external examination, or by looking down the throat, I proceeded to pass a bougie. There was a very slight impediment at the upper part of the œsophagus, which was overcome without difficulty, and the instrument was pushed on into the stomach. The effect was marvellous; the patient swallowed without the slightest difficulty immediately afterwards, and swallowed thenceforth as readily as he had always done up to the time of his illness. He came under my care again six months later for a temporary attack of catarrhal jaundice, of which he recovered in the course of a week or ten days. He had had no recurrence of dysphagia.

The third case was one of painful interest to me, for I failed to recognise its nature, and it is mainly, perhaps solely, to this failure, that his death must be attributed. The patient was a young man, whose illness had commenced six months before I took charge of him, and was attributed by him to his having drunk a pot of beer which irritated his gullet as it passed down. From that time he seems to have had constant sickness after food, and to have vomited from five minutes to half an hour after

everything he took. He was very thin and weak when I first saw him, but I was unable to detect any evidence of abdominal disease. I attached very little importance, however, to the attributed origin of his illness, and assumed that, as his vomiting was often (indeed generally) delayed for some considerable time after the ingestion of food, it was due either to pyloric obstruction, or to some functional disturbance of the stomach referrible to disease external to it. In other words, though I never ventured to commit myself as to the exact nature of his malady, I believed that he had organic disease, either chronic ulcer or cancer of the stomach, or disseminated peritoneal cancer or tubercle. There were obvious and strong reasons against each of these views of his case; still, believing as I did that the vomited matters came from the stomach, I did not see my way to any other explanation, and I never thought of passing a bougie or of feeding him by the œsophagus tube. His vomiting was persistent up to the time of his death. At the post-mortem examination his stomach and other abdominal organs were all found to be healthy, and the only lesion discovered was dilatation of the œsophagus with hypertrophy of its walls. I now naturally attached more importance than I had done to the history which he gave of his illness; I admitted that his dilated and flaccid œsophagus had formed a virtual impediment to the entrance of food into the stomach; I became impressed with the important practical fact that in œsophageal obstruction vomiting may be delayed for half an hour or more, as it is habitually in pyloric stricture; and, above all things, my unfortunate experience taught me the importance, in all obscure cases of persistent vomiting, of not omitting to examine the œsophagus, or to try the effects of injecting food directly into the stomach. The following are the details of this case:—

Dilatation of the Œsophagus with Persistent Vomiting. Death.

J. B., a gardener's labourer, aged 24, was admitted into St. Thomas's, under my care, on June 7, 1879.

He was perfectly well (he said) until six months previously, when one day he drank about a pint of beer, which had a bad taste, caused some irritation along the gullet, and made him

sick. The next day he had pain and difficulty in swallowing, and vomited after every meal. The vomiting had continued ever since, coming on from five minutes to half an hour or even an hour after ingestion, and induced not only by food, but by iced water and by medicine. He thought, however, that solids were less provocative of sickness than fluids. He had at no time, since the very first, had pain or difficulty in the act of deglutition, but had often suffered more or less pain behind the sternum and extending to the umbilicus, which came on after food and was relieved by vomiting. He had never vomited blood. He had generally had a desire for food, and had suffered only slightly from thirst. The bowels had been much confined. He had had a slight cough on and off for some time, and said that his sputa had occasionally been streaked with blood. He had lost flesh and strength latterly.

He was a thin, delicate-looking man, and patches of dilated vessels in his cheeks added to the unhealthiness of his aspect. His tongue was clean and moist, but rough, his appetite was fair, his bowels constipated. He complained of a slight dry cough; but on physical examination there was no evidence of disease either of the lungs or of the heart. The respirations were 18, the pulse 120, feeble, small, and regular. The abdomen was soft and flat, and no tumour nor enlargement of any organ nor tenderness was discovered in it. The urine was small in quantity, free from albumen, and its specific gravity was 1028. The temperature was subnormal. There was no œdema of the limbs, and no enlarged glands in any accessible region. The vomit consisted mainly of matters which had been swallowed, and presented no pathological products under the microscope. The motions were solid and of healthy character. He was treated with bismuth and put on milk diet.

It would be tedious, nor would it be instructive, to reproduce the periodical notes that were taken of the patient's case from the time of his admission up to August 25, the day of his death; for, beyond the fact that there were progressive asthenia and emaciation, the symptoms varied but little from week to week.

He was treated dietetically mainly, by milk, and latterly by wine in addition, given in small quantities by the mouth at

frequent intervals, and by nutrient enemata administered from two to four times a day. This treatment had the effect apparently of diminishing his sickness from time to time, and even of arresting it occasionally for a day or two; but on the whole the vomiting continued generally after everything he took, and from five minutes to half an hour or so afterwards, and at times was severe. The vomit was generally merely what he had swallowed mixed with mucus; but occasionally it was dark and offensive, and had an unpleasant taste. Streaks of blood were observed in it from time to time. He generally complained of pain behind the sternum after swallowing, and occasionally also of pain at the episternal notch, a few inches below the left nipple, or at the umbilicus. It was noted on one or two occasions that the vomit came up without any straining. And, during the earlier part of his residence in hospital, he manifested a desire for food. He complained but little of thirst. His tongue was generally coated and sometimes dry.

The abdomen never became tumid. On the contrary it got more and more hollow, and was always free from tenderness and evidence of tumours. Shortly before his death it was remarked that what appeared to be a narrow thick-walled tube could be felt extending transversely across the abdomen above the umbilicus, and could be freely moved upwards and downwards. It was assumed to be the contracted stomach.

The bowels were for the most part confined, but during the latter part of July the patient suffered from diarrhœa.

The daily yield of urine varied from 10 to 18 ounces. Its specific gravity was high, but it was free from albumen and other abnormal matters. At one time he complained of pain, and difficulty in passing it.

He suffered more or less from cough during the whole of the time he was under observation; and at times it was very troublesome and attended with mucous expectoration, which was occasionally streaked with blood. There was never any clear evidence of pulmonary phthisis, but respiration was harsher and the voice resonance louder at the right than at the left apex and some variable crepitation and rhonchus were observed here and there.

The heart's action was very feeble, and the pulse, which was always small and regular, sank until latterly it was only 52 in the minute.

His temperature never rose above 98·7°, and was almost invariably subnormal. It tended also to sink from the time of his admission to the day of his death. During July it ranged for the most part between 97° and 94° in the axilla. In August it sank still lower, and before his death fell to 92·8°. Latterly also he complained much of cold, and his hands and feet became cold and livid; occasionally he perspired. There was a uniform loss of body-weight from first to last. He weighed 8 stone 3 pounds when he first came into the hospital. He weighed only 5 stone 9 pounds on August 16. He was often very low-spirited during his illness, and was apt to cry; and shortly before his death he was occasionally delirious. He was conscious, however, to the last.

Autopsy.—The œsophagus was much dilated throughout its whole length, and full of fluid. It measured five inches in circumference at its upper part, and three inches just above the cardia. There was no stricture. The muscular coat was hypertrophied. The mucous membrane was thickened, generally pale, but presenting a few injected vessels, and thickly studded throughout its whole length with shallow circular pits, which appeared to correspond to dilated mucous follicles.

The stomach was much contracted, and its mucous membrane presented a few patches of congestion. The intestines were contracted and healthy. All the other abdominal viscera and the peritoneum were free from disease.

The lungs were congested, and the base of the right one was collapsed. Otherwise they were healthy. Heart healthy.

The lessons applicable to my hysterical patient, which these three cases taught, were that vomiting from the œsophagus might simulate vomiting from the stomach; that œsophageal spasm might form a persistent obstacle to the passage of food; and that small quantities of alimentary or other matters might fail to excite the proper peristaltic movements of even the healthy gullet, and that hence the feeding of patients with teaspoonfuls of food, as is done in cases of irritable stomach, might fail both

to impart nourishment and to throw any light on the condition of the patient's stomach.

The results of my experiment will appear in the narrative which I now proceed to give.

Hysterical Vomiting.[1] *Cure.*

A delicate-looking girl, 14 years of age, was received into one of my beds in St. Thomas's on September 21, 1881. She was suffering, we were told, from an hysterical affection of the right hip, which first showed itself in July, 1879, and which had continued, without cessation but with varying severity, up to the date of her admission. It did not appear that she had ever before suffered from any serious illness; she had never had fits; she had never been hysterical (in the popular sense of the term); the catamenia had not appeared.

At the time mentioned she fell into a state of languor and weakness, and the right hip became very painful—the pain running down the front of the thigh, and extending into the knee. The pain increased in severity until October, at which time she limped in walking, and only put her toes to the ground. This condition continued without change to the end of the year, when some improvement took place, which was maintained during the greater part of 1880. In the autumn of that year her symptoms became much aggravated; she seems to have had considerable lumbar pain, and is said to have had sciatica. She then took to her bed, and never left it till July, 1881. There was no improvement, however, after this time, and she was at about her worst when I first saw her.

Then she complained of some pain in the back, but mainly she suffered with her hip. The joint was slightly flexed, and when she attempted to walk (which she did unwillingly and only with assistance) the right lower limb was kept bent at the hip- and knee-joints, and she limped, but she planted her foot flat upon the ground. The joint was excessively tender, especially behind the greater trochanter, but there was no swelling, redness, or increase of temperature, and it was distinctly observed that she complained no more when the joint-surfaces were pressed

[1] An almost identical case of œsophageal spasm, only in a young boy, is recorded by Mr. Hulke. *Clinical Soc. Trans.* vol. vi.

against one another, or when the ligaments were stretched, than she did when the skin was simply touched. There was some rigidity about the joint, but no wasting of muscles. There was nowhere any loss of sensation or of the tendon reflexes, but the superficial reflexes were feeble. The affection had been regarded as hysterical, prior to admission, and in this opinion I and others who saw her in the hospital concurred. While under treatment she manifested a tendency to sob at times, and the condition of her hip varied a good deal, but she left apparently much improved on December 10.

She was re-admitted on May 15, 1882. It appeared that, soon after she left the hospital, she began to vomit after food, and before long, after everything she took, the sickness coming on immediately; that she rapidly lost flesh and strength; and that, although the hip-affection remained, it formed a less prominent subject of complaint than it had done previously.

She was much emaciated (weighing only 3 stone 3½ pounds), very feeble, and confined to bed; her cheeks were a little flushed, and her face (which rarely varied) wore a mixed expression of apathy and martyr-like resignation; her skin was dry; her pulse feeble and slow; her temperature normal. She vomited after everything she took; her bowels were constipated; her urinary secretion was normal; she had no abdominal pain. The belly was hollow, and presented neither tenderness nor lump. The catamenia had not appeared. Her hip was tender, and the joint was kept partly flexed; but she complained of it much less than when she was in the hospital last.

From the first she continued to vomit after whatever was taken; the vomit consisting mainly of the food swallowed and mucus, and the sickness generally coming on a few minutes after ingestion. It was sometimes, however, delayed for ten minutes or a quarter of an hour. It appeared, nevertheless, that a small proportion of her food was retained. After a day or two's experience she was ordered to take a dessertspoonful of milk only every half-hour, which she vomited. The quantity was then reduced to a teaspoonful, which she likewise vomited; but at the same time nutrient enemata were directed to be administered twice daily. As the vomiting continued without abatement after every kind of fluid swallowed, no matter what

its bulk, it was determined to make a trial of small quantities of solid food, frequently given. Pounded raw beef, mixed with currant-jelly, was selected, of which she took a teaspoonful at a time. This, however, returned as everything else had returned. A fortnight after admission (the rejection of food continuing unabated) it was determined for a few days to give the stomach entire rest, and to feed her solely with nutrient enemata, of which at first three and subsequently five were given daily. She was treated thus for about ten days, and during this time vomited only after taking medicine, which was consequently discontinued after a day or two. During the latter period no particular change was observed in her condition; she presented the same manner and appearance as on admission; she complained of no pain excepting in her back and right hip; she was generally very restless and sleepless at night; she had no desire for food or drink; she did not feel sick; her bowels were confined; her temperature was generally subnormal, as it had been nearly ever since admission, and often sank to 96°; her pulse, which was extremely feeble, ranged between 42 and 60. She had lost only one pound in weight in a little more than three weeks.

At the end of this time the administration of milk in teaspoonful doses was recommenced—at first only three or four times a day, and then every hour. But again we were disappointed; for after every dose of milk, milk was speedily vomited. In fact her stomach appeared to be just as irritable now as it had been at first. After a day or two a grain of opium in the form of a pill was given two or three times a day; but this treatment had no effect. The administration of milk by the mouth was persisted in for four or five days, at the end of which time, feeling a good deal puzzled and disheartened, I gave more serious thought to the incidents of her case than I had previously done, and discussed them fully with my class. I had hitherto assumed that she was suffering from extreme irritability of the stomach, and that it was this irritability which caused her to vomit constantly. But I now called to mind that she had never complained of actual pain or tenderness in the region of the stomach; that she was not flatulent; that her vomiting was an easy process with her; and especially that she brought back the greater part of the minutest quantities of

food taken, and in whatever form it was taken. And I asked myself the question, "Was it possible that the bulk of her food never entered the stomach at all, but was retained in the œsophagus and thence regurgitated?" I had other reasons, which I have already fully explained, which helped to suggest this question, and inclined me to answer it in the affirmative. I now made the girl swallow a dessertspoonful of milk in my presence, and watched the progress of events. She swallowed it without difficulty, and evidently it went beyond the influence of the pharynx; then she appeared to suffer from some discomfort, and in the course of a minute or so, without any very violent effort, but with a certain amount of spasmodic action, the milk was gulped up into the mouth.

I then (on June 11th) got Mr. Pitts, the resident assistant surgeon, to see the patient with me. And at my request, and in my presence, he passed a medium-sized india-rubber tube along the œsophagus into the stomach, and then injected into that organ about three ounces of milk. There was a little impediment to the passage of the instrument in the lower part of the tube; but it was readily overcome, and evidently was not due to any organic disease. The milk thus injected did not provoke any feeling of sickness; and remained in the stomach without causing discomfort. She did, immediately after the removal of the tube, regurgitate a small quantity of milk; but this was clearly only the milk which escaped into the œsophagus during the withdrawal of the instrument.

It was intended to feed her daily by the tube, but she never required it again during her stay in the hospital. For the next day or two she took milk in small quantities, returning a little of it only occasionally. Two days after the use of the tube, she began to take a tablespoonful of milk every hour, which she retained. The next day a teacupful of milk, with a little tea in it to give it flavour, was ordered to be given several times a day instead, and a little bread-and-butter was added. These also were retained. The next day her allowance of food was increased by a small quantity of custard-pudding; and thus by degrees her diet was improved in quality and increased in quantity until, at the end of two or three weeks (or about June 28), she was

taking daily a fair quantity of milk, together with two eggs, fish, pudding, and bread-and-butter. The nutrient enemata, however, were persisted in for a day or two longer, and were then discontinued, partly because their more nutritive ingredients had been withdrawn for administration by the mouth, partly because the bowels, which had hitherto been constipated, became loose. The diarrhœa troubled her for about a fortnight, and had to be treated by astringents. But she continued to take food in increasing quantities up to the time of her leaving the hospital.

It must be observed that the patient to the last appeared to have no desire for food, and to derive no pleasure or comfort from taking it. She took it only because she was compelled; but she took it without difficulty or discomfort. Only on one or two occasions did she vomit any of it. She did not gain flesh very appreciably, and, in fact, when she left only weighed two pounds more than she had done on admission, and three more than at her period of greatest enfeeblement and emaciation. The continued diarrhœa may, to some extent, have retarded her progress in this particular, but in other respects the improvement, if slow, was marked; she certainly got stronger and more cheerful, and her aspect and complexion assumed the characters of health. Moreover, her temperature, which for the greater part of her stay in the hospital had ranged between 95° and 98°, during the last few weeks seldom fell below 97°, and generally varied between one or two tenths of a degree above 98° and one or two tenths below it. The pulse was generally very slow throughout her illness, varying perhaps between 40 and 60, but latterly it rose occasionally to 70 or 80.

She left the hospital, on July 29, cured of the vomiting and generally benefited in health, but not so much benefited as I could have wished. The fact is she fretted so much, and so persistently, to go home, that at length, fearing her constant fretting might be retarding her convalescence, I reluctantly complied with her wish. It was clear, however, that though she was thus cured of one important outcome of her hysterical condition, the fundamental malady still remained. The hip-joint continued painful; and I was scarcely surprised to hear that, a month or two later, during my absence from town, there had been a recurrence of the vomiting, and that her mother had

brought her to the hospital to have the œsophagus-tube re-introduced.

I have little to add by way of comment. There is no doubt of course that in most cases of hysterical vomiting, it is the stomach that rejects the food. But it is obvious that in an undetermined minority of cases of such vomiting, of which my case is an example, it is the œsophagus rather than the stomach that is in fault, and if, in such cases, the irritability or spasm of the gullet can only be overcome, and the food swallowed be allowed to reach its destination, the vomiting will cease. If one has reason to suspect the latter condition to be the cause of his patient's symptoms, it is fortunately easy to put the question beyond doubt by having recourse to the œsophagus-tube or stomach-pump ; and, if the answer be in the affirmative, to cure the patient of her malady by the repeated use of the instrument and artificial feeding. There is reason, however, to hope that a single introduction may suffice to effect a more or less permanent cure.

How often one has reason to wish that the past with its misapprehended experience could be recalled! I have often thought, since I learnt the lesson which my hysterical girl taught me, that the two cases which I quoted at the beginning of my paper were cases of the same kind as hers, and might have been cured with comparative ease and rapidity.

ON THE USE OF CHROMIC ACID IN CERTAIN AFFECTIONS OF THE TONGUE.

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ALMOST by chance I have discovered a remedy for certain affections of the tongue which will prove most valuable in speedily removing them. Sir James Paget, in his *Clinical Lectures and Essays* (2nd edition, p. 373), says that a most useful local treatment of gouty psoriasis of the tongue is to paint the white patches with a saturated or very strong solution of chromic acid. Although the out-patient department of a large hospital does not afford a large number of cases of gouty psoriasis, it does bring many patients suffering from affections of the tongue, and among them a fair proportion of cases of chronic superficial glossitis, most of which are due to excessive smoking and drinking, often combined with syphilis. On some of these cases of glossitis I determined to try chromic acid in solution, and during June (1881) treated two patients with a ten-grain solution in water, which was ordered to be painted on the sore areas of the tongue three or four times every day. Both patients certainly improved under this treatment; indeed, one of them, who had been previously treated with glycerine of tannic acid and other remedies during several months without avail, was so much relieved in the week by the acid that he was lost sight of until the following October, when a fresh attack of inflammation brought him back to the hospital and discovered to us the reason of his disappearance.

At the same time that this man was attending the hospital I had also under my care a man (J. B.) suffering from obstinate

secondary syphilitic ulcers of the borders of the tongue, small but deep and jagged, and from ulceration of the inside of the left cheek. During April and May he took Hyd. c. Cret., until, at the end of two months, his gums became a little sore. Iodide of potassium was then substituted for the mercury and continued in doses of from ten to twenty grains until July, when the dose was diminished to five grains, and a dram of Liq. Hyd. Bichlor. was administered. He took this medicine regularly until October 17 (1881), but really without any improvement in the ulcers, which, indeed, looked worse and were larger than when I first had seen them. He was put on chromic acid solution merely as an experiment, and without much hope of affording him relief. But a week later (October 24) he returned with the ulcers almost completely healed. On October 20, before the result of treatment on J. B. was known, I ordered a chromic acid solution for a man (T. L.) who had a number of flat mucous tubercles, due to secondary syphilis, on the right border of his tongue. He had been treated with Hyd. c. Cret. gr. iii. from June until October, and with powdered calomel and other local applications without the slightest benefit. On October 27 his tongue was almost well, and in the course of between two and three weeks scarcely any trace of the tubercles remained.

Since that time I have employed chromic acid in the treatment of several kinds of inflammatory conditions of the tongue, and in certain cases with most marked success. I have notes of twenty-seven patients, twenty of whom have been cured or greatly relieved, seven have received little or no benefit. The seven cases were either of chronic superficial glossitis, or of *tertiary* syphilis. The twenty patients include seven suffering from chronic superficial glossitis, and thirteen from various *secondary* syphilitic affections. From these cases the following conclusions may be drawn: Chromic acid cures with marvellous rapidity *secondary* affections, ulcers, mucous tubercles, condylomata. It produces no appreciable effect on *tertiary* affections, gummata, extensive ulcers, tubercular syphilides. Some cases of chronic superficial glossitis, where slight ulceration and renewed inflammation has occurred, improve quickly under its influence. Other cases of glossitis, in which the tongue surface is attacked by a fresh inflammation of great severity, so far from

improving, appear actually to be rendered worse by chromic acid. Glycerine of boracic acid and soothing remedies are more suitable for such conditions.

Fortunately, the secondary syphilitic affections are those for which some rapid remedy is most needed, for the tertiary affections generally yield rapidly to large doses of iodide of potassium or to mercury, and they are probably not at all contagious, as the secondary affections undoubtedly are. A patient, whose tongue and lips are quickly cured of secondary syphilis, is not only relieved, so far as he himself is concerned, but he ceases to be a source of contagion to those with whom he associates, a source of contagion often the more dangerous in that neither he nor those around him are conscious of the danger. The only objection to the rapid curing of these affections, in hospital patients especially, is that they are unwilling, after the local mischief is relieved, to continue the long course of constitutional treatment which is necessary for the complete cure of their syphilis. Many of them withdraw themselves at once from further treatment.

In contradiction to the statement previously made that tertiary syphilis is not benefited by chromic acid, I may mention one case in which an excellent result followed its employment. A medical man of my acquaintance called on me last June to ask whether I thought that some small, ragged, and deep syphilitic ulcerations of the tip and borders of his tongue were likely to be transformed into epithelioma, of which he had a great and natural horror. The primary attack of syphilis had occurred at least ten or twelve years previously; it had been treated during many months with mercury and iodide of potassium. The ulceration of the tongue had been a source of great annoyance and distress to him during many months, although he had been under the care of several of the most eminent surgeons in London, one of them especially skilled in general surgery and syphilis. Bicyanide of mercury and caustics, with various constitutional measures, had been employed, but without avail, and he came to ask me—not to cure him, for he believed he was incurable—but whether he would die of cancer of the tongue. By my advice he took two and a half grains of Hyd. c. Cret. daily, and painted his ulcers with a solution of chromic acid. I neither saw nor heard anything of him until November, when he came to

show me his tongue, quite healed and much less scarred than could have been expected. Under the treatment it had quickly improved, and in less than a month was well. Six weeks later a second outbreak had occurred, which had given way to treatment in two or three weeks. There can be very little question that in this case the mercury had taken an important share in effecting a cure, but I do not think the mercury without the chromic acid would have availed, and certainly not with such rapidity. It may be that the acid acted well in this instance because the ulcers did not resemble typical tertiary ulcers, but rather the ulcers which attack the tongue in the later period of secondary syphilis.

The strength in which the chromic acid has been almost invariably employed has been ten grains to one ounce of water. In a few instances fifteen grains have been ordered. The patient has been told to paint the diseased portions of the tongue three or four times a day with a camel's-hair brush dipped in the solution. Pain or discomfort from the application has seldom been complained of; and even if there has been a little smarting at first, this has cheerfully been borne on account of the relief which the lotion has afforded.

Of the precise mode of action of the acid I cannot speak with certainty, partly because I have not been able to remove the healing parts in order to examine them, partly because an interval of several days, often a week, has elapsed between the dates of my seeing the patient. But I have observed that raised areas quickly subside until they reach the level of the normal tongue, and that the little red areola by which they are surrounded disappears and gives way to the normal colour of the mucous membrane. From this, perhaps, it may be assumed that the epithelium of the raised areas (mucous tubercles and condylomata) shrinks or is cast off, and that the vessels contract under the influence of the acid. Its action on ulcers is less apparent.

I hope at some future time to be able to afford more definite information on this point, and also to be able to describe the effect of chromic acid on syphilitic affections of parts other than the tongue and lips. At present I can only say that it is very useful in secondary affections of the tonsils and the palate.

NITRITE OF SODIUM IN THE TREATMENT OF ANGINA PECTORIS.

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It is now nearly two years since I procured a small quantity of nitrite of sodium with the object of trying it as a substitute for nitrohydrochloric acid in the treatment of certain forms of liver disease. In order to ascertain to what extent it might be safely administered, and whether it possessed any untoward action, I took on three separate occasions five, ten, and twenty grains of the salt. I then observed that the rate of my pulse became accelerated shortly after taking each dose, and most distinctly after the largest dose. But, what was more remarkable, I experienced within a few minutes after taking the two larger doses a feeling of fulness in my head and eyes, accompanied by a throbbing sensation. There was also a slight, almost doubtful, flushing of the countenance. The sense of fulness and throbbing continued for an hour or more after the administration of the salt, without at any time being so intense as to be unbearable, or even severe enough to prevent me from proceeding with my usual duties. Indeed, it was comparatively trifling, and caused me no inconvenience. The smallest dose of the salt produced a similar effect, but of very short duration and very slight in degree—so slight as almost to have escaped observation. These experiments were repeated on myself and a few of my friends, and always with the same result.

The similarity of the effects of nitrite of sodium, although less in degree, to those produced by nitrite of amyl and nitroglycerine at once suggested that the peculiar action of these

bodies was dependent on the nitrous acid present in them. The suggestion occurred the more readily, as I was previously aware that Gamgee[1] had shown that the chemical action of nitrite of amyl on the blood was identical with that of other and simpler nitrites, as, *e.g.*, the nitrite of sodium. And it is to its action on the blood that many investigators ascribe the physiological effects of the nitrite of amyl. Moreover, it appeared highly remarkable that two salts, as nitrite of amyl and nitro-glycerine, whose bases differ so widely in their pharmacological activity, should so exactly agree in their physiological action and therapeutical applications, were it not that the acid, which is common to both of them, was the essential factor in the production of their action.[2] As these substances had been found, the former by Lauder Brunton, and the latter by Murrell, to be of the greatest value in the treatment of angina pectoris, I thereupon resolved to try the therapeutic effect of nitrite of sodium in the first well-marked case of angina which might come under my care. Such a case did not present itself until November of last year. Since then, I have searched the literature of pharmacology to ascertain what, if anything, has been written on the action of a simple nitrite, or a nitrite whose base possesses, at any rate in small doses, little action of its own. My search has not been fruitless; for I have found that, in addition to Gamgee's paper and various other publications on the action of amylic nitrite and other ethereal nitrites, there exist accounts of three researches on the action of metallic nitrites by Barth, Binz, and Reichert and Weir Mitchell respectively.[3]

[1] Gamgee, *Philosophical Transactions*, 1868, p. 589.

[2] I here assume that nitro-glycerine is, in respect of its action on the body, a nitrite, although its chemical formula actually represents it as a nitrate of glyceryl, $C_3H_5.3NO_3$. I am at present investigating its chemical composition.

[3] Since the above was written, Dr. Lauder Brunton has kindly informed me that so early as 1870, whilst investigating the action of nitrite of amyl in Ludwig's laboratory at Leipzig, and a few years later, in conjunction with Mr. Gresswell, in his own laboratory at St. Bartholomew's, he made observations on the action of nitrite of sodium, comparing its action with that of nitrite of amyl, and became convinced that they resembled each other, but was prevented from recommending the sodium salt as a substitute for the amyl compound in therapeutics, owing to the former apparently possessing a much less powerful action than the latter. These experiments are unpublished.

Barth's investigation[1] was conducted in the laboratory of Binz, and had for its object the finding of an explanation of the occasional poisonous action on cattle of Chili saltpetre, or nitrate of sodium, which is now largely used in agriculture as a manure, and which therefore finds its way into the fodder of the cattle. Barth attributed the poisonous effect to the presence of nitrite of sodium, the nature of the toxic action of which he examined on frogs, rabbits, and other lower animals. In the following year Binz[2] published a short article on the action of nitrite of sodium, which formed a continuation of Barth's investigation. Their experiments were made on both cold- and warm-blooded animals, and in every case the nitrite of sodium produced a decided and, if the dose were large enough, a lethal effect. The animals first became drowsy and giddy, and frequently there were fibrillar contractions of the muscles, and, especially in dogs, vomiting and yawning. Very shortly afterwards, along with a browning of the colour of the blood, the respiration became deep and laboured, and death occurred, unpreceded by spasm or convulsion. The nervous tracts were evidently paralysed; and paralysis of both muscles and nerves certainly occurred in frogs. Rabbits and dogs were frequently purged by a large dose of the nitrite, even when injected subcutaneously, and, if the dose were lethal, the mucous membrane of the intestinal canal was often found to be reddened and inflamed. The minimum lethal dose, injected subcutaneously, for a rabbit of average weight was about three grains. A dog, weighing nine pounds, was killed in less than four hours and a half by a dose of four grains subcutaneously administered. Binz attributes the irritating action of the nitrite on the intestinal canal to the acid of the salt being there set free and becoming decomposed, forming nitric acid and nitric oxide. Neither Binz nor Barth points to a similarity of action between the nitrite of sodium and the nitrite of amyl.

In the same year in which Binz's paper appeared, and almost in the same month, Reichert and Weir Mitchell[3] published the

[1] Barth, *Toxikolog. Untersuchungen über Chilisaltpeter.* Diss. inaug. Bonn. 1879.

[2] Binz, "Ueber einige Wirkungen des Natriumnitrits," *Arch. f. exper. Pathol. u. Pharm.*, Bd. xiii. S. 133.

[3] Reichert and Weir Mitchell, *Americ. Journ. of Med. Scien.* N.S., 1880, vol. lxxx. p. 158.

results of a tolerably exhaustive and evidently carefully conducted research on the physiological action of potassium nitrite, from which they concluded, as my own observations had led me to anticipate, that the action of potassium nitrite was almost identical in its nature with that of nitrite of amyl. They observed the effects of the salt on man as well as on the lower animals, and arrived at the following conclusions:—That the salt possesses a very feeble narcotic influence on the brain of mammals, more marked on that of the frog, and that the convulsions are clonic in character and cerebral in origin; that it depresses and finally completely paralyses both the motor and sensory portions of the spinal cord, acting much more quickly on the former; that it diminishes the function of the motor and sensory nerves, ultimately paralysing them; that it primarily increases the pulse-rate, and secondarily diminishes it, and at the same time lessens the force of the pulse; that it primarily raises the blood-pressure on account of a direct action on the heart, and secondarily lowers it by causing vasomotor and cardiac paralysis; that the respiratory centres are first stimulated, and afterwards depressed, and that death is due to paralysis of the respiratory centres, when not dependent on cardiac paralysis; that it at first slightly elevates the temperature, and afterwards considerably depresses it; that it finally paralyses the voluntary muscles, and merely impairs the function of the involuntary muscles; and that it primarily stimulates, and secondarily depresses, the heart. Dr. Reichert, Dr. Weir Mitchell, and Mr. Hinsdale, a student in the University of Pennsylvania, took at various times doses of nitrite of potassium varying from three to ten grains, and on one occasion Mr. Hinsdale took thirty-five grains in the course of six hours. In no instance were alarming effects experienced. The pulse was always considerably increased in rapidity, this effect beginning to be observable in from one to two minutes after taking the nitrite, and continuing from one to three or four hours. After a few minutes a slight flushing of the face was sometimes perceived, accompanied by a gentle feeling of warmth in this and other parts of the body, and by a slight fulness of the head along with some throbbing of the cranial arteries, especially if the dose exceeded five grains. The authors concluded that nitrite of potassium is nearly identical in action with

nitrite of amyl. It is unfortunate that they employed in their experiments the potassium and not the sodium combination; for the sodium is *per se* practically inactive, whilst the potassium acts powerfully on all muscular structures, the heart included, especially when injected subcutaneously, or directly into the circulation, as happened in their experiments on the lower animals. Still, if we make a careful comparison of the action of potassium nitrite with that of nitrite of amyl, eliminating such effects as might be due to the metal of the former salt, we cannot but in the main agree with these observers in their conclusion, that the two compounds resemble each other in their physiological activity, and that their activity is due to the nitrous acid, which is common to both of them. Reichert and Mitchell are wrong in stating that these two bodies are *identical* in action, for both of their bases, the potash and the amyl, possess distinct and well-characterised actions of their own. Yet the effect upon which the therapeutical value of nitrite of amyl is supposed to depend—the dilatation of the smaller arteries and lowering of the blood-tension—follows the administration of either salt; and in this important respect, therefore, the two salts agree in action.

The only case of angina pectoris in which I have as yet used, and, I am happy to say, with success, the nitrite of sodium was a well-marked case of this disease. T. B., aged forty-two, married, a letter-press printer, consulted me on the 15th of November, 1882, complaining of severe and prolonged spasms of pain in his chest occurring several times a day, and even disturbing his rest at night. His father died suddenly of heart disease at the age of fifty-two, but never suffered from pains in his chest. His family history is otherwise unimportant.

Until within the last year, the patient does not recollect of having any illness except the usual fevers incidental to childhood. His father told him that he was a difficult infant to rear, that he was late in learning to walk, and that shortly afterwards he lost the power of walking, which he did not recover until after several months. He was apprenticed to printing at the age of eleven, and six years later began to work a hand-press, which he continued to do for nine years, and which required great bodily labour. Ever since then he has worked at a

steam-press, where the exertion needed is much less. About six and a half years ago, whilst doing some strong and sudden pulling in connection with the press he racked himself, and for some weeks afterwards had a peculiar feeling about the epigastrium and substernum as if the stomach were pressing upwards. Two years ago, whilst dancing, he felt a peculiar pain in his chest, which passed off tolerably quickly and did not again recur. There is no history of rheumatism, acute or chronic. He is a man of fair muscular development, and his expression is careworn and anxious.

The patient attributes the commencement of his present illness to an excessive indulgence in both food and stimulants at a dinner with some friends on the 1st of January, 1882. He felt very sick and vomited next morning, and continued to feel sick in the morning and vomit occasionally for nearly three months afterwards. During all this time he suffered from a constant pain, often very severe, in the region of the stomach; and for the purpose of relieving the pain he used to take from one to three glasses of whiskey in the course of the day. But finding that the pain began to get worse, he consulted his usual medical attendant, who, of course, ordered him to abandon stimulants, except a glass or so of beer in the week. With this important modification of his habits, and with the use of the medicines prescribed, in the course of other two or three months he felt considerably better, and the pain had almost entirely gone. About the middle of June, however, whilst walking hurriedly along the street after dinner, he felt a peculiar and severe pain at mid-sternum, quite different in character and situation from the pain he had formerly experienced. The pain gradually went off as he continued walking, but it recurred in the evening whilst walking up hill home from his work. I may here mention that the patient resides fully half a mile from the printing-office where he is employed, and that he has to mount one long and steep hill on the way from the office to his house. At this time it was his custom to walk home for both breakfast and dinner. Until the end of June he repeatedly felt this peculiar pain in his chest, generally on walking up the hill on his way for breakfast and dinner and in the evening, and frequently also on returning to his work after dinner. The pain was gradually

getting more severe, and shortly after the beginning of July he began to experience it after rising in the morning as soon as he commenced to put on his clothing, and also on his way to his work in the morning. Indeed, a small amount of exertion at any period of the day seemed sufficient to initiate it. By the end of July, or nearly six weeks from the time he first observed it, the pain had become so severe and actually excruciating that, whenever it occurred whilst he was walking in the street, he was obliged to stand for a few minutes until it became less severe, and sometimes he had to stop almost every twenty yards on his way up the hill. He now ceased going home for dinner, and a week or two later had also to give up going home for breakfast. But the spasms of pain continued to recur as strongly as before, whenever he attempted to walk a short distance or when he exerted himself unusually at his employment. A tolerably severe spasm was also felt after dinner, without the patient having exerted himself in any way. This was his condition, only that the pains were still more severe and more frequent in their occurrence, when he came to consult me on the 15th of November last. He was quickly losing flesh and strength, and was barely able to continue at his employment. He had been prescribed for by more than one physician, receiving at one time ethereal stimulants and hyoscyamus, at another time digitalis and iron, without deriving the slightest benefit. On his own responsibility, besides a selection of patent medicines, he had tried large doses of quinia for a time, but also without effect. Sometimes, with the object of relieving the pain, he took a glass of whiskey, sometimes a glass of beer, and occasionally both together; but whilst they questionably helped to deaden the pain, they did not prevent its returning shortly afterwards with as much severity as ever.

On the 15th of November, when I first saw him, the following was the condition of his circulatory system. The radial pulse was fairly strong, moderately full, regular in force and frequency, and eighty per minute, and there was no perceptible rigidity of the radial or any other superficial artery. The heart was slightly hypertrophied, and its impact against the chest-wall at each pulsation was somewhat stronger than usual. Both of the heart-sounds, as heard at the apex, were soft and prolonged; in

the aortic area the first sound was followed by a short, soft, blowing murmur, and the second by a louder and more prolonged murmur of the same character. There was evidently a lesion of the aortic valves.

The patient described each attack of the pain as beginning in the middle of the front of the chest and extending over a space of about the size of the hand. It gradually extended backwards until it was distinctly felt between the shoulder-blades, and it passed down both arms even to the finger-tips, and in the arms was particularly severe in the right wrist. He was generally made aware of its approach some one or two minutes before it was actually felt by a peculiar, uneasy sensation in his chest. The pain, once it had commenced, lasted generally for nearly fifteen minutes, but sometimes for five or ten minutes only, and was quite excruciating. Just before the pain began to abate, the patient felt a throbbing between his shoulders at the back of his chest. The pain passed off quickly and entirely. Whilst it lasted he experienced no sense of suffocation or difficulty in breathing, no sickness, and no giddiness. I examined his urine, and found that it was natural and contained no albumen or sugar. There was no arcus senilis, and there was no dropsy of any kind. The pain from which he suffered was evidently a well-marked angina pectoris, as we commonly understand it.

The treatment I recommended was twofold—for the angina, three minim capsules of nitrite of amyl to be broken and the contents inhaled whenever an attack supervened or was impending; and for the removal of some gastric irritation, of which there was still some evidence, and which probably arose from chronic gastritis, I enjoined regulation of diet, abstinence from all alcoholic stimulants, and the use of a sedative mixture which I prescribed for him. He called again on the 30th of November, looking and feeling much better. The gastric irritation was gone, and the pain had been greatly relieved by the nitrite of amyl. He had used the capsules at the rate of six or seven a day, indeed, whenever he felt the pain commencing—one in the morning immediately after rising, another on his way to his work, a third after, and sometimes before, dinner, a fourth before leaving for home at night, which enabled him to

walk home without suffering any pain, and a fifth at bedtime, with an occasional one during the night; for, latterly, he had begun to have one and, generally, two attacks of the pain during the night. Besides these, there were other occasions, as when working more actively than usual, or when out walking, that necessitated his using the capsules. The inhalation of the nitrite did not entirely dispel the pain, it merely dulled or deadened it, and it was always accompanied by giddiness, which compelled him, if he were at the time in the street, to stand for a minute or two, and it was followed by a headache and disagreeable feeling which lasted for one or two hours. He was, however, extremely grateful for the relief from severe pain which the nitrite of amyl had been the means of giving him. It was at this time that, recollecting what I had some time ago thought as to the action of nitrite of amyl being for the most part due to the nitrous acid, I resolved to make a trial of nitrite of sodium, which, if it were followed by satisfactory results, would offer therapeutical proof of the correctness of my supposition, and at the same time furnish, in addition to nitrite of amyl and nitro-glycerine, for the treatment of a distressing disease, a valuable remedy, inasmuch as it contained the nitrous acid in its simplest possible combination, uncomplicated by the presence of a base possessing an undesirable or disagreeable action. I therefore gave my patient the following prescription :—

℞. Sodii nitritis, oz. $\frac{1}{2}$.
Aquam ad fl. ozs. xii. Solve.
Sig. Dose, one to two teaspoonfuls.

As I did not feel assured that the nitrite of sodium would prove serviceable, I asked him also to procure some nitrite of amyl, but in bulk, and to inhale five or six drops of it, as I was under the impression the quantity in each capsule was not sufficient to produce the desired effect. The expense, too, of the capsules, where so many were required, formed a consideration. The nitrite of amyl was only to be resorted to in the event of the nitrite of sodium proving inefficient.

The patient returned on the 8th of December with an extremely favourable account of the benefit he had derived

from the use of the nitrite of sodium. He had taken, as I had suggested, a dose a few minutes before rising in the morning, and the result was that he was enabled to get up, dress, breakfast, and walk to the printing-office, without experiencing the slightest pain, and without requiring more of the nitrite until after dinner. He tried to do without it one morning, but the pain at once came on when he commenced to dress himself. After dinner he took it only when he felt the pain was coming on, and if he took it promptly it caused complete cessation of the pain in from one to two minutes. Nitrite of amyl never completely abolished the pain, not even when he inhaled from ten to twenty drops of it. It is possible, however, that the preparation he obtained in bulk was not good, for even one of the three minim capsules which he formerly used, had a stronger effect than twenty drops of it, although in no instance had the nitrite of amyl given him that complete freedom from pain which the nitrite of sodium did. Another important difference, he said, was that the nitrite in the dose prescribed caused no perceptible throbbing in any part of the body, and certainly no headache. Apart from its effect on the pain, it seemed, he added, to have no more action than so much water. If he required to take it in the street, he could do so without having to stop for a few minutes afterwards and without feeling in the least giddy. In addition to taking it in the morning and after dinner, he took a dose always before leaving for home at night, and another at bedtime. He still continued to waken once or twice during the night with the pain, which a dose of the nitrite of sodium at once arrested. He felt very greatly pleased with his new remedy, for not only was it preventing or relieving the pain, but his general health was improving under its use (perhaps because it kept him free from frequent, almost intolerable, pains), and he felt fitter for his work than he had done for nearly two years previously. Certainly his general appearance was greatly improved.

The next time I saw the patient was on the 21st of December. His general health was still improving, and the nitrite of sodium still continued to completely relieve, or rather prevent, the spasms of pain. Whenever he omitted to take the nitrite he found that he was liable to attacks of the pain almost as

severe as before he came to consult me. If he took none of the medicine immediately after rising, he never failed to have an attack of pain when he commenced to dress himself, and another on his way to his work.

In order to ascertain if the effect of the nitrite of sodium was more prolonged than that of the nitrite of amyl, I asked him to again procure some capsules of a pure preparation of the latter, and on four consecutive mornings to alternately take a dose of the former and inhale the contents of a capsule of the latter, and to observe within what time in each instance an attack of pain followed. The medicine was to be used immediately before rising, and before any symptom of pain was experienced. At the end of the fourth day he returned to report that whilst on each morning in which he had inhaled the nitrite of amyl he was able to dress and breakfast without suffering from the pain, yet on both occasions a severe attack of it supervened after breakfast, when on his way to the printing-office.

On the other hand, a single dose of the nitrite of sodium had kept him perfectly free from pain until after dinner—that is, until between two and three in the afternoon. The preventive action of the nitrite of sodium is, therefore, exercised over a much longer period of time than that of the nitrite of amyl.

I next asked him to compare the two as to the power and rapidity with which each of the two medicines arrested the spasm of pain, if the medicine were not taken until the fit of pain had commenced, or had given indications of its supervening. After having compared them in this respect more than once, he came back to tell me, that, as I had expected, the nitrite of amyl acted more promptly than the nitrite of sodium, but that, in the doses taken, the latter, when once its action had asserted itself, which occurred in from two to three minutes, completely relieved the pain, whereas the former merely dulled the pain, and did not shorten its duration. The nitrite of sodium acted more quickly than I anticipated, and when taken, as he informed me, during the monitory stage preceding the actual onset of the angina, the pain was prevented from occurring.

The next occasion on which he came to see me was on the 6th of January of the present year. His general health had been steadily improving, and his appetite for food and his

capacity for work had not been better for several years previously. The pains were still continuing to be felt unless the nitrite of sodium were taken, but they were not quite so severe, nor did they last so long as formerly. At the commencement of the year he had resumed his old custom of walking home for his dinner, and by taking a dose of the medicine immediately before starting for home, and sometimes a little immediately after dinner, he was able to accomplish the walk home and back in perfect comfort. He still continued to be awaked by an attack of the pain during the night, but now never oftener than once, and generally between 3 and 4 A.M. During the day he took the nitrite almost as frequently as before, and at the times I formerly mentioned, with an extra dose on any occasion when, from having to do his work more hurriedly than usual, or after running up stairs, he felt that the pain was about to come on. On one or two evenings, as I had advised him, he took at bedtime a large dose—three to four teaspoonfuls—of the medicine, in order to try if it would altogether prevent the occurrence of the pain during the night, but although each dose produced immediately after being swallowed a throbbing through his whole body and a dull beating at the vertex of his head, yet he was awakened as usual by the pain about 4 A.M., when he had to take another dose of the medicine. Beyond the throbbing and beating, the large doses caused no other inconvenience; there was no headache or pain, and only a very slight giddiness. It may be as well to mention that the patient, in taking an ordinary dose of the solution of the nitrite, never measured it exactly, but swallowed a small mouthful from a vial of the solution which he always carried with him. By ascertaining how many doses he was able to get from the bottle, and the capacity of the bottle, I calculated that each dose corresponded very closely to two teaspoonfuls, or between four and five grains of the salt. In order to observe roughly the physiological effect of such a dose on the patient, I asked him to take one in my presence.

2.55 p.m.	Pulse, 85.	3.4 p.m.	Pulse, 84.
2.57 p.m.	,, 84.	3.6 p.m.	,, 89
3.0 p.m.	Took fl.ʒii. of solution of nitrite of soda.	3.12 p.m.	,, 88
		3.17 p.m.	,, 84.
3.2 p.m.	Pulse, 86.	3.20 p.m.	,, 84.

Before taking the nitrite, the pulse was firm and regular, and tolerably full. After taking the nitrite, it became a little more rapid, and seemed to feel slightly, almost doubtfully, softer and smaller to the touch, but was certainly not greatly altered in character. There was no appearance of flushing, and the patient felt no throbbing and no unpleasant sensation. Immediately afterwards, I took the same dose of the same solution myself, but without observing any effect. The pulse was altered neither in character nor in frequency. It is certainly remarkable that a dose of the nitrite of sodium which produces so little apparent physiological effect should so thoroughly relieve the patient from an excruciating pain. On this occasion, for the sake of comparison and without saying anything whatever as to its probable action, I asked the patient to try nitro-glycerine for a few days instead of nitrite of sodium, and I prescribed it for him in the following simple form :—

℞. Sol. nitro-glycerini (1 %) fl. ʒi.
Aquam ad ... ... fl. ℥vi. ♏.
Sig. Dose, one to two teaspoonfuls.

He returned four days afterwards to tell me that the new medicine, which he had used to the entire exclusion of the sodium salt since I previously saw him, acted quite as well as the latter, completely preventing or relieving the spasms of pain, but that the taking of each dose was always followed by a distinct throbbing in the head accompanied by some degree of pain and giddiness. He found that its power of warding off an attack of angina lasted for a much longer period than that of the nitrite of amyl, but in this respect was hardly equal to the nitrite of sodium, although the difference was not great. For example, a single dose (ʒij) of the nitro-glycerine solution taken early in the morning enabled him to get on until before dinner without the supervention of the pain ; but a single dose of it taken before starting home for dinner never enabled him to accomplish the return journey without being subject to the pain, whereas a single dose of the solution of the nitrite of sodium was generally sufficient under similar circumstances to permit of his comfortably accomplishing the double journey. At

his own request I permitted him to at once return to the use of the latter.

Since then, until now (8th February), with the exception of a day or so at a time, when he was trying at my suggestion some other combinations of nitrous acid, and whose effects I may make the subject of a future communication, he has continued to use the nitrite of sodium, and he finds that it has not been necessary, in order to obtain the desired effect, to increase the dose, although two days ago, for the purpose, if possible, of hastening the cure of the angina, I have asked him to double the dose.

Towards the end of January I asked him to ascertain the smallest dose which would prevent or relieve each attack of pain, giving him certain instructions for the exact attainment of that object. He found that neither ten, twenty, nor thirty drops of the solution taken after rising prevented the pain coming on before breakfast, but a teaspoonful did; and that the smaller doses merely dulled but did not completely relieve the angina, once it had commenced. A dose, therefore, of the nitrite of sodium of about two grains is evidently sufficient in his case. And even a much smaller dose of a *pure* preparation of the salt may suffice, for, knowing that the nitrite of soda, unless carefully manufactured, is a little apt to be mixed with the nitrate, I procured a quantity of it two days ago from the druggist who was supplying my patient; and I confess I was somewhat astonished to find that, although obtained from a London manufacturing firm of the highest eminence, the specimen analysed did not contain more than 33 per cent. of the nitrite, the remainder consisting of the nitrate of sodium. This is a very important fact, for its knowledge should serve to prevent failure in the therapeutical use of the nitrite of sodium from the preparation not being what it is supposed to be. If sufficient care is taken in its manufacture, it is not difficult to obtain the salt in an almost pure form. Another specimen of the nitrite which I procured for myself nearly two years ago contains only a trace of the nitrate. The proportion of nitrous acid is easily estimated by means of a standard solution of permanganate of potassium. Allowing, therefore, for the impurity of the preparation used by my patient, the requisite

or efficient minimum dose of the nitrite would seem to be in his case a little less than one grain.

The present condition of the patient is thoroughly satisfactory. He very rarely has any pain during the night, although he often wakens about six in the morning with a desire to urinate. The spasms of pain are still apt to come on in the morning and during the day, but never unless after exertion. Formerly they even occurred when he was sitting quietly by his own fireside. When they do come on, and if he does not check them by having recourse to the nitrite, they are very much less severe than they once were, and are now quite bearable, and never last for more than three or four minutes instead of from five to fifteen minutes. His pulse is full, strong, and regular; the cardiac sounds are unaltered. The urine has never exhibited any trace of albumen or glucose, substances which sometimes appear after the use of nitrite of amyl and nitro-glycerine.

I offer no opinion as to the exact cause of the angina pectoris in this particular case; nor shall I say anything as to the nature of the beneficial action of the nitrite of sodium, beyond remarking that the good effected is evidently accomplished by a very small alteration of the dynamics of the circulation. I may, at another time, discuss the nature of this action.

My principal object has been a therapeutical study of the value of nitrite of sodium, and, incidentally, of some allied substances, in angina pectoris. I greatly regret that the clinical material has been so little; but although only a single case, even had I had many more at my disposal, I would with difficulty have found one better adapted, either by the severity of the pains, or the regularity and frequency of their occurrence, for the purpose of crucially testing the value of the nitrites. The patient was, moreover, a man of considerable intelligence, which greatly facilitated my experiments. It is quite clear from the results of the treatment of this case that the active remedial part of nitrite of amyl, nitro-glycerine, and nitrite of sodium is the nitrous acid. The suggestion of this is not now new, nor is there any novelty in its practical application, for, based on the analogy of the physiological action of nitrite of potassium to that of nitrite of amyl, Weir Mitchell,[1] of America, was the first to

[1] *Op. cit.* p. 163.

recommend and try the use of a simple alkaline nitrite in the treatment of epilepsy, and later, in this country, Law,[1] Saundby[2] and Ralfe,[3] the two former in this Journal, have recorded cases in which they have employed it with varying success. But as the preeminent therapeutical action of the nitrite of amyl, for the knowledge of which we owe a great debt of gratitude to Lauder Brunton, rests with its use in the treatment of angina pectoris, it is particularly in this disease, against which all other remedies seem to be powerless, that we would expect to obtain the most valuable field for the employment of some simple combination of nitrous acid. And it is extremely satisfactory to find from the case related, which, to my knowledge, is the first case of angina pectoris in which a simple nitrite has been employed, that in the nitrite of sodium, the simplest of all the compounds of nitrous acid, we have a remedy as active in kind and as reliable in effect as nitrite of amyl or nitro-glycerine, and possessing several distinct advantages over either of these in respect, among other things, of producing in therapeutic doses no disagreeable general effects—headache, giddiness, and even partial collapse. Whether the nitrite of sodium is the combination of nitrous acid most to be preferred on pharmaceutical grounds is yet to be determined. The nitrite of potassium would act equally well, but it is rather more deliquescent than the sodium compound. The taste of the latter somewhat resembles that of common salt. The principal objection to the use of the nitrite—but, fortunately, one of little moment when we consider the nature of the disease which it is used to alleviate—will probably be found in the eructations of nitrous acid gas which it is apt to provoke, especially if it be necessary to give it in large doses, but from which the patient in the present case never suffered, owing to the small dose administered. The nitrites, on account of their remarkable physiological action, will, I venture to anticipate, in the form of their simple and more safely administrable compounds, soon have a wide and important application in the treatment of various forms of disease.

[1] *Practitioner*, June 1882, p. 420.
[2] *Ibid.* Feb. 1883, p. 105.
[3] *Roy. Med. and Chir. Soc.* Nov. 28, 1882.

ON AN OPERATION FOR THE CURE OF DETACHMENT OF THE RETINA.

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THE anatomical relation between the choroid coat and retina may be disturbed by an effusion of serum between the two membranes. The effusion may be so slight that it cannot be detected even by the ophthalmoscope, so that we have in our diagnosis to depend entirely on the subjective symptoms. The patient complains that an opaque spot intervenes between him and the object when he looks in a certain direction, whilst there are no *muscæ volitantes*, and no tremor of any part of the retina is visible. In some cases again the obscuration extends over a larger spot and then the ophthalmoscopic mirror shows undulation, or a wrinkled portion or fold of the retina caused by some fluid underneath. The fold is generally transverse, but sometimes it runs obliquely and becomes visible under the mirror when the patient moves his eye in various directions. The effusion may accumulate until a bluish-grey mass screens part of the fundus, and the retinal vessels are seen crossing its surface. It may even increase in size until very little of the fundus remains visible. The detachment may be in one spot only, or in two different places, and occurs more frequently in the lower part. Even when it begins in the upper portion it generally gravitates downwards, and the originally detached portion of the retina again adheres and resumes its functions.

Retinal detachment occurs more frequently in one eye only, but may occur in both, and may be the result of simple serous

infiltration without change of structure, or may be complicated with such structural changes as softened vitreous, disorganisation of the retina, apoplexy, or other disease of the choroid. It may supervene idiopathically, or may be the result of an injury by a blow on the head, wound, contusion of the eyeball or of the bones of the orbit.

When the detachment is limited to one spot of the retina, the patient may see perfectly well, and may even be able to read small print when the eye is rotated in such a manner as to displace the fluid, but when it becomes extensive, vision is interfered with. As the margin of the detachment approaches the macula, objects appear slanted or crooked, and when the macula is invaded there is eccentric fixation, *i.e.* a neighbouring portion is used for that purpose, and the visual axis deviates in that direction. When the effusion gains ground and screens the entire fundus, vision becomes entirely abolished. In this aggravated form the disease was known, even before the discovery of the ophthalmoscope, under the name of subretinal dropsy, but its existence could be recognised only when the retina was much elevated and floated behind the crystalline lens, where it could be seen with the naked eye in the form of a bluish mass, whilst by the aid of the ophthalmoscope we can discover it at its early stage, watch its progress, and determine its various forms.

The cause of the idiopathic form of the disease is generally obscure. In some cases, however, it can be traced to a chill caused by a sudden exposure to cold after being heated. This is a fruitful source of mischief, and plays a most important part in all forms of internal inflammation. Desmarres[1] records the case of a lady of distinction who, in coming from a ball at the Hôtel de Ville on a frosty night, was unable to find her cloak. Too impatient to wait for it, she walked with bare head and shoulders to her carriage, and was instantly taken with detachment of the retina of the right eye. I can recall analogous cases from my own experience.

Indeed the vascular relation between the skin of the face and the retinal vessels, as well as the nervous supply of these parts, will account for the phenomenon in question in a case

[1] *Maladies des Yeux*, iii. p. 479.

of chill to an overheated face and neck. With regard to the treatment of detachment of the retina, the ophthalmoscope has, until very recently, only revealed to us that we were in the presence of an affection which we must put under the category of incurable diseases.

Von Graefe introduced an operation in which he punctured the retina with a needle, and allowed the fluid to burst into the vitreous or into the choroid. Mr. Bowman performed the same operation with two needles, but I am not aware that it had ever been adopted by any other surgeon.

De Wecker tried to drain off the infiltration by means of gold wires introduced through the sclerotic, but had to abandon the procedure.

Having observed, in cases of wounding of the sclerotic and prolapse of the vitreous, how satisfactorily the wounds heal without prejudice to vision if we secure coaptation, I have come to the conclusion that the safest operation for removing the fluid is to cut down upon the sclerotic and evacuate the effused serum by puncture.

Before resorting to operative interference we must ascertain (1) that there is no opacity or softening of the vitreous, or at any rate, if present, that it is not general but is confined to the region of the detachment. (2) That the retina is healthy. (3) That the effusion is purely serous. We must then determine the exact site of the detachment, so that the lance enters the pendent portion of the retinal bulging.

Operation:—The patient being put under chloroform and the speculum introduced, the assistant fixes the eyeball with forceps. A vertical slit is made with scissors into the conjunctiva and subconjunctival tissue, laying bare the sclerotic at a point corresponding to the site of the detachment, which is generally below the equator at its anterior aspect (Fig. 1).

The lips of the wound are separated by two small strabismus hooks, and the assistant steadily maintains the position of the eyeball to prevent the exposed portion of the sclerotic from shifting. The sclerotome (Fig. 2) is introduced into the sac formed by the fluid. The incision through the sclerotic is made obliquely in such a manner that the edges of the scleral wound should overlap each other when the instrument is withdrawn and

not remain gaping. Gentle pressure is made upon the eyeball in the track of the receding lance by means of a fine spatula. The lips of the external wound are brought together with a fine silk ligature or two, and both eyes are strapped with court plaster. The patient is kept in bed in a dark room for three days. The plaster and ligatures are removed on the sixth day,

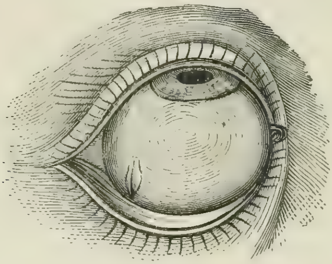


FIG. 1.

and the eye is gradually accustomed to light. On the eighth day the result of the operation may be tested.

CASE I.—My first successful case was recorded [1] as follows:—

Miss A. L., æt. 58, shopkeeper, who had previously enjoyed perfectly good health and sight, felt her eyes beginning to get dim in Oct. 1877. When she consulted me in January following.

FIG. 2.

I found in the right eye retinal dropsy complete, and the left progressing in the same direction. In May the detachment became complete in the left also, vision was entirely abolished, blindness being so advanced that she could not follow the flame of a gas-burner passed within a few inches of her eyes. I operated on the right eye on September 15 in the manner described above, with the result that, on the eighth day, she could distinguish persons distinctly, and count figures in all

[1] *The Lancet*, October 12, 1878.

directions at two feet distance. On the twenty-second day after the operation she could see the time to a minute on a white-dialed watch, and distinguish between shades of colours such as red and pink, green and blue. Six weeks after the operation she called at my house, having passed through a crowded thoroughfare unaided. The following year I received a letter addressed to me in her own handwriting.

CASE II. was reported by a former assistant, Dr. Cappie-Shand.[1] Mrs. G., æt. 47, applied at the Ophthalmic Institution in the beginning of December, 1881, for deficiency of sight. Ophthalmoscopic examination showed opacity of the vitreous of the left eye, and in the right complete detachment of the retina with pigment floating in the subretinal effusions. She could not see the light of a gas-burner held before her, but could discern only a faint glimmer of the burner at the lower and outer margin. Tension slightly increased ($T = 1$). This was a case of detachment of the retina complicated with choroiditis. In the middle of January she had an attack of iritis in the blind eye. The sclerotic was punctured on Feb. 19, 1882. About half a drachm of yellow serum was withdrawn along with the black pigment. On Feb. 23, when the dressings were removed, she could see everybody in the house, could count fingers and distinguish features and different colours. A cursory ophthalmoscopic examination showed the disappearance of the serous effusion, but a considerable wrinkling of the retina, especially at its lower periphery, while the centre was tolerably free. The remarkable circumstances of the case, noted at the time, were the entire absence of pain or feeling of uneasiness in the eye since the operation, but rather a feeling of greater comfort in it, as well as the healthy appearance of the conjunctival sac. Indeed, with the exception of redness at the spot where the incision had been made, the rest of the conjunctiva bore no trace of an operation.

CASE III. was reported by my present assistant, Dr. A. T. Thomson.[2]

A. B., æt. 14, was admitted into the Glasgow Ophthalmic Institution in May, 1882, for detachment of the retina of the right

[1] *Medical Times and Gazette*, March 11, 1882.

[2] *Medical Press and Circular*, August 16, 1882.

eye. The boy appeared healthy, no history of previous ailments, although he looked rather pale; he enjoyed perfectly good sight until within six months before, when it was noticed that the right eye suffered from convergent squint; about April the schoolmaster began to complain that the boy held his book very close to one eye and his head twisted in that direction; and in the beginning of May the failure of vision terminated in almost total blindness. Ophthalmoscopic examination on admission revealed detachment of the retina limited to the lower and outer part, while the optic disc and the upper and inner segment of the fundus were quite visible. This apparently healthy part of the fundus presented a fine subretinal infiltration. He could see the shadow of a finger moved downwards and inwards, but in the upper and outer side of the field he could not distinguish even shadows nor point out the movements of a burning lamp. As the abolition of sight was out of all proportion to the retinal detachment, the case was considered as affording little encouragement for surgical interference. The operation was however resorted to in July, 1882, with the result that on the fourteenth day after the operation he could see to move about the house, when the other eye was closed, and could read No. 20 of Jaeger, with perfect ease. The patient was examined three months later, when it was found that the improvement of his vision was still maintained.

CASE IV. was published by Dr. Thomson.[1]

A. L., æt. 29, stonemason, admitted into the Glasgow Ophthalmic Institution, August 7, 1882. He always enjoyed good health and sight until the middle of January of last year, when dimness of sight came on suddenly one morning. Has been addicted to taking whiskey in excess. On the margin of the left orbit on its temporal region there is a vertical cicatrix caused by a wound with a piece of iron, which was inflicted three years ago. He applied to two ophthalmic hospitals for relief, in both of which his case was pronounced incurable; from one of these he holds a card stating that he suffers from "amblyopia potatorum and separation of the retina." He was indeed greatly addicted to drink, but as the other eye was never affected we regarded it simply as a retinal detach-

[1] *Medical Press and Circular*, October 11, 1882.

ment. Ophthalmoscopic examination showed general infiltration of the retina of the left eye, but there was in addition one prominent central portion more elevated than the rest. This central rag of the retina was floating in the effusion. The operation was performed on the 9th August, 1882, in the usual manner, but instead of making the incision in the vertical meridian, it was done rather in the outer angle in the interval between the inferior and the external recti muscles. The fluid was withdrawn by a puncture, and the lips of the wound brought together by a silk ligature. August 13th, fourth day after the operation, vision was restored to such an extent that he could see objects distinctly in every direction, could distinguish also between a florin or shilling and a penny-piece, read No. 20 Jaeger, and see to walk about when the right eye was closed. Ophthalmoscope shows still the small central floating detachment, which has only slightly been reduced in size, but the rest of the infiltration has disappeared, and the greater part of the fundus is perfectly visible. I have operated in other three cases with similar good results, and in no instance has there been any reaction or prejudicial effects from it.

As the operation has not yet been adopted in this country,[1] I think it desirable to bring the subject again before the profession; especially when we consider the harmlessness of the operation, and the great advantage which we derive from it in cases which would otherwise be considered incurable, as seen in the following case in which I recently operated.

Case V.—Mr. James L., æt. 72, farmer, consulted me ten years ago about the condition of his right eye, which I pronounced incurable, and at present this eye is totally blind, and the pupil closed by posterior synechia. Six years ago he had an alarming attack of epistaxis which lasted for eight days with intermissions. The left eye has been serviceable until October 1882, when blindness came on, and advanced rapidly until the beginning of January of the present year, when perception of light was almost completely lost. He could follow the light of a

[1] One London surgeon, indeed, tried it in two cases. He put in Graefe's knife in one place through the soft parts, and no fluid having come out, he put it into another, when welling under the conjunctiva took place. When the operation is thus carried out in the dark, a satisfactory result is scarcely to be expected.

lamp at a few inches' distance only at the lower part of the field, but upwards and inwards not at all. The retinal detachment rendered the state of the fundus totally invisible, pupil not dilatable beyond one-half, tension normal.

I performed the operation on the 24th January. Chloroform was administered by Dr. Thomson, but when about two drachms were used he had to discontinue the anæsthetic on account of sudden cessation of the pulse and lividity of the face. So completely and rapidly was anæsthesia produced, that an incision was already made through the inferior rectus, and the sclerotic exposed. The patient rallied as quickly, and the remainder of the operation was performed without chloroform. About one drachm of serum was withdrawn.

January 29th.—Fifth day after the operation. The eye was opened and examined. He can see and count fingers in all

Fig. 3.

directions. Can distinguish features of all parties in the house, sees small objects.

February 1st.—Ninth day after the operation. Very little mark of an operation visible, and vision steadily improving. He can describe minutely the dress and ornaments of persons, and can move about independently of assistance. Tension is normal; has so far recovered as to be able to return home.

With regard to this operation, when the patient is under chloroform, the steadiness of the eyeball can be properly maintained, but without it there is a risk of the eyeball rotating, and the incision made getting out of sight. To guard against such an occurrence, and to keep the eyeball under complete control, I have devised the instrument shown in Fig. 3.

It is put like a thimble upon the index finger of the left hand of the operator, and the point, which is provided with a stop, is pushed through conjunctiva and sclerotic, and keeps the eye steady.

History of the operation.—After the publication of my first

case in the *Lancet* as an original operation founded upon clinical observation in cases of injury to the sclerotic, I became aware that my excellent friend, Prof. Hirschberg, operated successfully for detachment of the retina the preceding year, and published some results in the *Centralblatt* with his characteristic lucidity and thoroughness. In my work on diseases and injuries of the eye, therefore, I have given to him the credit of priority. In a letter, however, which I have received from Prof. Sattler, that eminent ophthalmologist states: "Among the German surgeons to Prof. A. Graefe is due the credit of having first performed the operation in the summer of 1876 and published it in the beginning of 1877. There can be no doubt that you hit upon the same idea totally independently of Prof. A. Graefe, as often occurs to eminent surgeons of different countries."

I may state, however, that although the idea is the same, my operation differs from Graefe-Hirschberg's procedure, for, whilst in their method the eyeball is rotated and the puncture is made through the conjunctiva, fibrous capsule, and other soft parts, and the serum is allowed to well under the conjunctiva to be absorbed, in my operation the fluid is withdrawn entirely. The one is an operation done under cover, in the dark, and may prove a miscarriage; my operation is done in open light, and can be managed with more precision.

Medical treatment.—In bringing before the profession the merits of surgical interference in this disease, I cannot omit referring to the medical treatment which has lately been recommended, namely, the hypodermic injection of pilocarpine.

M. Dianoux,[1] after having experienced twenty-nine failures in thirty cases, has ultimately succeeded in curing seven in eight cases by the systematic hypodermic injection of pilocarpine. The injections are administered to an extent so as to produce ptyalism, but without perspiration. The reason for this discrepancy of result is stated by himself: "Dieu sait si c'est la médication mise en usage (mercure et iodure) qu'il faut en faire honneur!" The reason why I did not refer to it in my work on *Diseases of the Eye* is that I reported only what has proved successful in my own experience, and that, as a rule,

[1] *Archives d'Ophthalmologie*, November, 1880.

I look with suspicion upon potent drugs like these. As, however, it appears to be in favour just now, I cannot pass it by unnoticed here.

Very recently, Dr. Landesberg[1] has experimented with that drug, and his report goes to throw some doubt on its ultimate success. Dr. Landesberg states that in four cases of detachment of the retina, and one of serous choroiditis in which the crystalline lens was perfectly transparent up to the commencement of the treatment, it afterwards rapidly became opaque. The result was the same in the case of a horse, which he treated by the infusion of Jaborandi leaves and injecting pilocarpine under the skin. The morbid process was rapidly arrested, but during the fourth week of treatment the crystalline lens was observed to become opaque.

In the incipient stage, however, or in cases where operative interference is contra-indicated, the drug deserves a cautious trial.

[1] *Philad. Med. Times*, July, 1882.

Reviews.

A Treatise on the Physiological and Therapeutical Action of Sulphate of Quinine. By OTIS FREDERICK MANSON, M.D. Philadelphia: Lippincott and Co. 1882.

QUININE is so important a remedy that the mass of literature relating to it is very great, and in a special treatise on the subject, such as the present, we naturally expect to find a tolerably comprehensive statement of what is at present known regarding the drug, as well as the author's own observations. In this expectation we are disappointed, for we find that recent researches on the subject seem to be ignored in the text; nor is any bibliography given as an appendix to supply the want. The older works are much more fully referred to, and although the title-page bears the date of 1882, we should have been inclined, from internal evidence, to say that the book had been written in 1862, and was now published almost without alteration, excepting the introduction of one or two cases of later date. Some of the cases quoted in the book are sufficiently interesting and instructive, and had the title been simply "Notes on the Action of Quinine" we might not have objected to its appearance; but those who get it with the expectation of finding either the physiological or therapeutical action of quinine at all fully treated will be much disappointed.

NEW EDITIONS.

1. *Quain's Elements of Anatomy.* Edited by Professors ALLEN THOMSON, SCHÄFER, and THANE. Ninth Edition. Two vols. 8vo. pp. 747, 947. 1194 illustrations, many coloured. London: Longmans. 1882.

THE new features of this classic are a thorough revision of the topographical part by Professor Thane, with the addition of colour in the figures of the blood-vessels; the improvement and

amplification of the sections on histology by Professor Schäfer; and the embodiment of recent acquisitions in embryology by Professor Thomson. The anatomy of the central nervous system corresponds much more nearly than before to the present state of knowledge. The histological and embryological parts are enriched by fairly full references to the literature of the various subjects. Some account is given of the researches on nuclear subdivision by Flemming and others. These changes appear in the fact that the ninth edition is richer than the eighth by 260 pages and nearly 200 cuts. It is now more worthy than ever of its high position as the standard Anatomy for English-speaking students.

2. *Treatise on the Theory and Practice of Medicine.* By J. S. Bristowe, M.D., F.R.S. Fourth Edition. 8vo. pp. 1218. Illustrated. London: Smith, Elder & Co. 1882.

A fourth edition of this well-known and much-used text-book follows within two years upon the third. Dr. Bristowe takes the opportunity to include such new subjects as have risen to notice within that time. Thus there are articles on acute ascending spinal paralysis, the attenuation or mitigation of contagion, and Koch's Bacillus tuberculosis. About forty new woodcuts illustrating pathological points are given; and the useful pages on the legal management of lunatics, which were so great an improvement to the third edition, have been revised by Dr. Bucknill. What the volume has lost in handiness it has certainly gained in fulness.

3. *The Essentials of Bandaging,* &c. By Berkeley Hill, M.B., F.R.C.S. Fifth Edition, revised and enlarged. Post 8vo. pp. 341. London: Smith, Elder & Co. 1883.

Mr. Hill's useful little work has been enriched by some short hints on laryngoscopy by Dr. Poore, and on ophthalmoscopy by Professor Tweedy. Among matters purely surgical, we find for the first time some mention of Thomas's splints, Croft's plaster-splints, and Carr's splints for wrist-fractures. The book also contains a concise summary of "Listerian details"; of surgical landmarks; and of the methods of restoring the apparently drowned. We have known the directions for passing the female catheter, as here given, to cause hopeless embarrassment to a beginner; the parts to be taken by the right and left hands should be more clearly distinguished. The house-surgeon and dresser have already shown how they appreciate Mr. Hill's handy book: we commend the new edition to practitioners who need a friendly reminder of how to do common things the best way.

4. *Handbuch der gesammten Arzneimittellehre.* By Professor TH. HUSEMANN of Göttingen. Second Edition, revised, in two vols. Vol. I., pp. 516. Berlin: J. Springer. 1883.

THIS work is drawn up with special reference to the second edition of the German Pharmacopœia, and forms a kind of commentary upon it. But the commentary is so full and searching, so *gründlich*, that scarcely anything of note in any language but English would seem to have escaped the author's ken. It is in fact an encyclopædia-in-little of the most recent continental work in pharmacy, pharmaceutical chemistry, materia medica (officinal or not), pharmacology, and therapeutics. A feature worth noting is the numerous approved prescriptions which are cited: they are naturally given in metric measure, but their simplicity and elegance will often make it worth while for the insular physician to take trouble to translate them for the benefit of the insular apothecary. We hope the second volume will contain a full index.

5. *Clinical Lectures on Diseases of the Urinary Organs.* By Sir HENRY THOMPSON. Sixth (Students') Edition. 8vo. pp. 175. Price 2*s.* 6*d.* Illustrated.

6. *The Diseases of the Prostate: their Pathology and Treatment.* By Sir HENRY THOMPSON. Fifth (Students') Edition. 8vo. pp. 157. Price 2*s.* 6*d.* Illustrated. London: Churchill. 1883.

WE have pleasure in calling attention to these new and wonderfully cheap editions of Sir Henry Thompson's widely known and widely valued works. They are not merely flimsy reprints. They are genuinely new editions. The clinical lectures contain all the additions and improvements which the author has introduced into his treatment of the subject up to last session, including "Lithotrity at a sitting." The "Diseases of the Prostate" contains an entirely new chapter on recent modes of relieving the sufferings of patients with advanced prostatic trouble. All students and many working surgeons will thank the author for his courage and consideration in starting a venture in medical publishing like that which has succeeded so well in other departments—the issuing of classical works at "popular" prices.

BOOKS ON NURSING.

1. *Notes from Sick-rooms.* By Mrs. LESLIE STEPHEN. Fcp. 8vo. pp. 52. London: Smith, Elder & Co. 1883.

MRS. STEPHEN has clearly not written because she wished to make a book, but quite simply because she had something to say. And that something is so much matter of her own

experience in sick-rooms, experience gathered by a quiet yet quick eye, that it has all the charm of freshness, though it bears on details that are familiar or should be familiar to all good watchers of the sick. She does not lay down any large rules as to nursing, she has nothing to say of the special qualities needed in the hospital nurse or field nurse, but she points out how some of the disagreeable circumstances attendant upon illness at home may be diminished or removed. Nothing better, simpler, quieter, or kinder, can be put into the hands of any one who is called upon to tend a sick friend. The true nurse will respond at once to Mrs. Stephen's way of putting things, and be grateful to the physician who introduces her in a time of need.

2. *First Aid to the Injured.* Five Ambulance Lectures by Professor ESMARCH. Translated from the German by H.R.H. PRINCESS CHRISTIAN. Fcp. 8vo. pp. 100. London: Smith, Elder & Co. 1882.

PROFESSOR ESMARCH'S lectures are of the type which the Order of St. John has made familiar in this country. There is perhaps rather more allusion to the battle-field, and to the principles and practice of antiseptic surgery, than would have been found in lectures given in England; but this in itself is a reason for placing them in the hands of those who are attending "ambulance classes." The Princess Christian has done her part admirably. The translation is smooth and idiomatic, and the simple but earnest spirit which has moved her to undertake it for the benefit of her often too helpless countrywomen deserves high praise. If her name should lead still more of them to acquire the rudiments of knowledge and skill that are so needful in accident "until the doctor comes," all true physicians will owe her thanks for so using her gifts and her position.

3. *Diet for the Sick;* being nutritious combinations suitable for severe cases of illness. By Dr. J. J. RIDGE. Second Edition. 16mo. pp. 54. London: Churchill. 1882.

NO good doctor is above the study of cookery, so far at least as relates to the diet of the weak or ailing; some we know go farther and trench on gastronomics as an art. Dr. Ridge's clear and simple receipts will often stand the practitioner in good stead who desires to give definite instructions as to the food of his patient, instead of merely advising "light," or "nourishing," or "milk" diet. The variety of wholesome "milk" foods and of pleasant drinks suggested is specially useful: it is here that one oftenest feels one's ingenuity taxed to "ring the changes" agreeably.

Clinic of the Month.

Cancer of the Vermiform Appendage.—This affection is so rare that a case reported by Prof. Thiersch deserves special mention. The particular case has a further point of interest, inasmuch as the tumour became very early adherent to the abdominal wall, and so was from the first supposed to be directly seated there. This view was strengthened by the absence for three years of any intestinal disturbance. At the time of admission to the hospital the tumour was as large as the fist, ulcerated on the surface, and pulsating with the iliac artery. It moved with the belly wall; and a funnel-shaped opening at the base of the ulcer admitted the finger into an internal cavity. This was taken for the lumen of the ascending colon, chiefly because an excised piece of the neoplasm showed that it was an intestinal adenocarcinoma. The operation which was undertaken proved very difficult, owing to the relations of the great vessels with the growth. The cavity in the tumour was then found to be continuous with the much dilated vermiform process. A tumour as big as a walnut was found at the junction of the process with the cæcum, and necessitated the removal of a piece of the intestine three inches long by an inch and a half wide. The patient died in 36 hours. *Post mortem,* a small quantity of effused lymph was found around the sutures in the intestine, no liquid effusion appeared, and the retroperitoneal glands were cancerous. Death resulted, therefore, from exhaustion. (*Berl. klin. Wochenschrift,* 41, 1882.)

Hernia reduced by Electricity.—Dr. Suprunenko mentions an experience of interest in the *Wratsch* (No. 17, 1882). A right inguinal hernia, strangulated for three hours, had resisted half an hour's taxis. A moderately strong induction current was then tried; the positive electrode being pressed against the tumour, while the negative was applied first against the lumbar vertebræ, afterwards over the umbilicus. The hernia at once began to diminish, and in less than two minutes disappeared entirely. Another case is given in the same journal

(No. 40, 1882). An old man of eighty had suffered from a strangulated hernia for twelve hours. Persistent taxis had altogether failed though Dr. Pergamin kept it up for over two hours. The induction current was then used for fifteen minutes, the pole being applied to various parts of the tumour, but this also failed. The current being still maintained, he again attempted manipulation, and in two minutes the bowel returned into the abdomen with a gurgling noise. (*Centralb. f. Chirurgie*, Dec. 9, 1882.)

Extirpation of the Gall Bladder.—Dr. Langenbuch, of Berlin, has added to the triumphs of modern surgery by successfully removing, by operation, the gall bladder for chronic cholelithiasis (*Berliner klinische Wochenschrift*, No. 48). The idea that this organ could be removed without endangering life was supported by its known congenital absence in some cases, and by the fact that elephants and horses do not possess the receptaculum for bile. The practicability of the operation was tested on the dead body. The value of the procedure as a remedy rests on the teachings of modern pathologists (Frerichs, Schüppel) that the gall-bladder is the seat, *par excellence*, of the formation of biliary calculi. The necessity for such surgical interference was thought to be proved by the occurrence of cases where the individual seemed to pine away, simply as a consequence of repeated pain, even under the influence of morphia, and of dietetic and other modes of treatment. Further the insidious manner and manifold directions in which gall-stones may migrate must be borne in mind. Dr. Langenbuch having thus thought out and worked at the subject, an opportunity for putting his conclusions to a practical test was not long wanting. A man, aged forty-three, who had been greatly reduced in health, strength, and flesh by repeated attacks of biliary colic, consented to become the first on whom the operation of extirpation of the gall-bladder should be performed. A T-shaped incision through the layers of the abdominal wall, the cross-piece corresponding to the lower border of the liver, and the vertical part being parallel with the outer border of the right rectus abdominis, was first made, and the peritoneal cavity laid open. The gall-bladder was then seen, its duct found and ligatured by catgut, and the bile drawn off from the bladder by a Pravaz's syringe; the organ was then carefully dissected off the under-surface of the liver. We have not thought fit to give the details, because a surgeon wishing to do the operation would, or ought to, first practise it on the dead body. In Langenbuch's patient there was a little venous bleeding from the under-surface of the liver; this was stopped by catgut ligature, and caused no trouble. There was apparently no escape of bile into the peritoneal

cavity. The patient did extremely well after the operation, and, in fact, recovered without any bad symptoms, if we except a little dry pleurisy on the fourth day. There had been no return of pain up to the middle of November, and the patient had gained very considerably in strength and weight. It may be mentioned that there were only two gall-stones, each of the size of a millet-seed, in the gall-bladder, and that there was some difficulty in getting an action of the bowels after the operation. So far as we know, this operation has never been previously carried out or even suggested. The surgery of the gall-bladder seems to have been limited to dilatation of fistulæ and the extraction of stones, with the opening of abscesses. *A priori,* it might have been thought that the excision of the gall-bladder would not have influenced the production of gall-stones. It might have been argued that there was a vice in the bile secreted which led to the crystallisation out of biliary calculi, this habit no doubt being favoured by the existence of places wherein it can stagnate, and where it may meet with agents (*e.g.* mucus) which would tend to separate its elements; and these considerations may serve to explain a good result, such as we record. It need not be said that the future history of Dr. Langenbuch's case will be looked for with great interest. The operation will probably only be called for in a small minority of cases, and at all events, should not be practised until all other measures have failed. (*Med. Times and Gaz.*, Dec. 9, 1882.)

Apomorphia in Cases of Poisoning.—Dr. Amand Routh illustrates the value of apomorphia as a ready and safe emetic in poisoning by two cases from his own experience. We have several times already insisted on the usefulness of this drug, discovered by Matthieson and Wright, and was first introduced as an emetic by Dr. Gee in 1869. Dr. Routh says:—"Those liable to be called to cases of poisoning are always glad to have an agent handy which, not in itself lowering, will produce prompt emesis, especially in those cases where the jaws are rigidly clenched and the stomach-pump absent or inadmissible. This agent I am sure we have in apomorphia, an alkaloid which Dr. W. Murrell has brought before the profession. Though a derivative of morphia, it has no narcotic effects in the doses required to cause emesis. Dr. Murrell recommends it to be kept in a solution of 1 in 50 strength, and to be given subcutaneously in doses of from $3\frac{1}{2}$ to 10 minims ($\frac{1}{15}$ to $\frac{1}{5}$ grain). Emesis occurs in from two to five minutes, the contents of the stomach being usually voided in one rush without previous nausea, but with violent and visible muscular action of the stomach walls. The following two cases will serve to show its utility:—

Case 1.—I was sent for to see Mrs. S., who was said to have swallowed a white powder and to be then dead. I found her on the floor, doubled up, jaws and hands clenched, blood and froth at mouth, respiration seemed absent, and pulse barely perceptible. She had not vomited. Though evidently dying, I injected five minims of the above solution into her arm, keeping my hand on the pulse. In two minutes and a half by the watch the stomach evacuated its contents with a rush, whilst the pulse seemed to rally for an instant and then finally ceased. Oxalic acid was proved to have been the poison used, and at the post-mortem about two drachms only of fluid were found in the stomach.

Case. 2.—A lady, a dipsomaniac, had obtained access to the wine-cellar and had swallowed straight off two bottles and a half of brandy. She then put the corks in her pocket, hid the bottles, put on her clothes, and went out for a walk with her footman. She walked quite steadily for 300 yards, when she dropped down insensible, and was carried home in a cab. On arrival, ten minutes after, I found her comatose, not able to be roused, respiration stertorous and infrequent, pupils dilated and insensible, jaws clenched, pulse slow and intermitting, two or three beats in every eight. Her stomach was full of fluid. I injected $3\frac{1}{2}$ drops of the solution, and in exactly three minutes and a half about a pint of alcoholic liquid was expelled, and altogether in about five minutes a quart (measured) of hardly-altered brandy was vomited. The pulse and respiration now improved, the pupils becoming slightly sensible, and I left her for two hours, by which time she could be roused temporarily. After twelve hours' sleep she awoke none the worse.

Apomorphia fails to cause emesis during chloroform narcotism, but no other drug seems to be antagonistic to it, and there is no reason why it should not be used to get rid of even morphia itself. In the dyspnœa of chronic bronchitis, emesis from apomorphia produces temporary relief. If only the certainty, rapidity, and absolute safety of apomorphia were known, it would undoubtedly form part of every practitioner's paraphernalia." (*Lancet*, Dec. 23, 1882.)

Mercury in Intestinal Constriction.—From an examination of a large number of cases, Bettelheim has come to the conclusion that the use of mercury in bulk (200 grammes as a dose) is by no means a worthless remedy. On the contrary it sometimes saves life in cases of obstruction of the intestine, not yielding to other means, which are due to fæcal accumulation, ascarides, twisting or introsusception; and no injury and especially no perforation of the intestine is caused by it. He therefore recommends that after the use of the ordinary means, such as moderate doses of laxatives, opiates, irrigation of the

intestine, changes in the position of the patient, electricity and massage, mercury in bulk should certainly be had recourse to without fear. (*Deut. Archiv f. klin. Med.*, p. 53, vol. xxxii.)

Picric Acid in Erysipelas.—Dr. Flaminio Tassi of Siena has used a saturated solution of picric acid in the treatment of four cases of erysipelas. It was painted on with a brush over the inflamed part. It appears to have a beneficial action, but the number of cases is too small to enable any definite opinion as to its therapeutical value to be formed as yet. (*L'acido picrico nella cura dell 'crisipela* (pamphlet), Torino, 1881).

Hysteria in Boys.—M. Charcot has just published a short lecture upon hysteria in boys. The author remarks that hysteria may occur in its fully developed form in the male sex as well as in the female. Seventy-seven instances of male hysteria have recently been collected in a thesis by M. Klein; and Briquet asserts that it occurs in the proportion of one male to twenty female cases. Charcot then gives in detail a well-marked case of hystero-epilepsy in a youth of seventeen. The attack commenced by an epileptic seizure with tonic and clonic convulsions, most marked on the left side, with loss of consciousness, but no biting of the tongue. Then the body was bent backwards (opisthotonos), and the patient at length opened his eyes, giving a cry of fright, and the attack ended with laughing and crying. In addition there was left hemianalgesia, and a *point hysterogène* in the left sterno-costal region. Tonic treatment and hydrotherapy sufficed together to effect a cure. M. Klein states that in the male sex hysteria is met with most commonly above the age of twenty-four; Charcot thinks it is commonest from twelve to thirteen years. He describes two cases recently under his observation, and the details of the second case are as follow:—A young Jew of thirteen had suffered for a year from headache, which became worse daily at five o'oclock, and ended in an attack of convulsions. The facts upon which the diagnosis rested were the (1) persistent cephalalgia, with a spot of exaggerated sensibility on the vertex; (2) the recurrence of the attack always at the same hour; (3) between the attacks, left hemianalgesia (to pricking, cold, and faradisation), with impairment of taste, smell, and hearing on that side, with diminution of the field of vision, chiefly on the right side, and on this side with colour-blindness for every colour except red; (4) zones of hyperæsthesia upon the cranium (*zones hysterogènes*). He was ordered to be separated from his relative, to be treated with tonics, electricity, and hydrotherapy. In four or five days the attacks became less marked, and in fifteen days disappeared. Soon the *zone hysterogène* disappeared, and at the

end of a month there remained only traces of the amblyopia. (*Progrès Médical*, Dec. 23, 1882.)

Cauterisation of the Clitoris in Hysteria.—The late Professor Friedreich shortly before his death had prepared a paper which has since been published on this subject. In many cases of obstinate and severe hysterical affections he has found that cauterisation of the clitoris by nitrate of silver has had the most beneficial effects. The cauterisation must be severe, as slight superficial cauterisation tends rather to aggravate the disease. The pain is at first severe, and during it the patient must remain in bed. Amongst the cases which he gives as cured with extreme rapidity by this method are—one of paraplegia, which had lasted for a year and a half; hysterical aphonia, lasting for two years; glossoplegia, lasting for four months; tonic spasm of the spinal accessory, lasting for seven months; and several cases of general severe hysterical convulsions. (*Virchow's Archiv*, p. 220, vol. 90.)

Extracts from British and Foreign Journals.

Action of Various Substances on Muscle.—Dr. Kobert has investigated this subject in two ways; first in reference to the alterations produced in the muscle-curve, and secondly in reference to the amount of work which a muscle is able to yield before and after poisoning. There are several circumstances which must be taken into account in considering the results of the experiments. The temperature is of the greatest importance, and a rise of 2° or 2½° C. often produced an increase of 10 to 30 gramme-metres in the muscular work; only when it rose above 20° R. (25° C.), and was long continued, did it lessen the work. Light was also important, and frogs which had been sitting before a window often gave better results than those which had been kept in darkness. A very great difference also was observed between frogs which had been sitting still or jumping about. A frog which escaped and made a few jumps was useless for experiment, for sometimes the muscles yielded a much greater amount of work than usual in consequence of the circulation in them having been increased, and sometimes they were quite exhausted by the effort. The water in which the frog had been sitting had also a great influence, and when it was dirty the work which the muscle yielded was greatly lessened. In experiments with curara, he found that this poison, by lessening the activity of the heart, greatly diminished the power of the muscles. Very different doses of this drug were required to produce equal paralysis in different frogs, so that there seems to be a great difference in the individual resisting power of these animals to the poison. When the muscle was completely exhausted by stimuli repeated every two or four seconds, its power was to a great extent restored by the injection of simple salt-solution into the lymph-sac. There are a number of poisons which do not alter the form of the muscle-curve, but considerably the amount of work it is able to yield. One of these examined by the author is copper. Others are zinc, apomorphia, saponine, cyclamin, asclepiadin, sanguinarin, delphinin. These had been

examined by other authors, and Kobert did not make any experiments with them. He tested, however, the effect of antimony, and found that it lessens muscular power, but only when large doses are employed, and when one waits for several hours after poisoning before examining the muscle. Another group of muscular poisons contains digitalin, digitalein, digitaleresin, digitoxin, toxiresin, scillain, helleborein, oleandrin, adonidin, neriodorin and neriodorein. Tanghinia, thevetin, and frynin, or toad poison, probably also belong to this class. A third class of muscular poisons comprises veratria, and substances having a similar action, such as antiarin. The author's experiments showed that although the muscles no longer contracted normally in a frog poisoned by veratria, and showed in a marked degree the rigidity characteristic of poisoning by this drug, they could still yield a normal amount of work. When the dose was very large, however, the irritability of the muscle was completely destroyed. Arsenic, like antimony, acts slowly on the muscles when injected subcutaneously, and in many cases appears to have no action whatever upon them when they are merely tested by the application of a faradaic current. When the absolute amount of work which they can yield is determined, it is found to be diminished by the poison, especially when it is injected directly into the veins. Platinum has an action like arsenic, and probably the same is true for mercury. Potassium and chloride of ammonium also lessen muscular work, while soda does not. Cinchonin produces marked diminution in the muscular irritability. Oil of mace also paralyses muscle. Harnack had found that lead produced a peculiar kind of poisoning in muscle—not paralysing it absolutely, but causing it to respond by very unequal contractions to equal stimuli regularly repeated. Only two poisions, emetin and cocain, were found by the author to have a similar action. Tartaric acid in small doses has no paralysing action upon the muscle; nor has tin, even when used in large doses. Creatin causes a rapid recovery of the muscle after exhaustion by work. Hypoxanthin has a similar action to creatin, but is rather less powerful. The action of these substances is exceedingly interesting, because they are important constituents in beef-tea, and these experiments show that in all probability beef-tea is not, as many suppose, merely a pleasant stimulant to the sense of taste, but is an important agent in increasing muscular power. Caffein also increases the capacity of the muscle for work. This increase occurs quickly, lasts for a considerable time, and is similar to that produced by creatin. This experiment gives us a reason why people who are doing hard muscular work not only take soup, but also tea and coffee. Caffein appears besides, from the observations of other experiments, to shorten the latent period in muscle. Glycogen

increases the capacity of the muscle for work. Iron in minute doses sometimes is beneficial to the muscle, but in larger doses is a muscular poison very little less powerful than arsenic. Alcohol in small and moderate doses does not diminish muscular activity; large doses diminish it greatly, though only for a time. Physostigmin has been found by Harnack and Witkowski to increase the irritability of muscle for slight stimuli, but the author did not find it to have any influence in increasing muscular activity. In large doses it considerably diminished it. (*Archiv. f. exper. Path. u. Pharm.*, Bd. xv. p. 22.)

The Micro-organisms of Typhoid.—Maragliano, of Genoa, has published, in the *Centralblatt für die med. Wissenschaften* (No. 41, 1882), an important note on the uniform occurrence of organisms in the blood of patients suffering from typhoid. He has found them in the blood of the spleen as well as in that of the general circulation. The blood was obtained by means of a hypodermic syringe, the needle of which was passed through the abdominal wall into the substance of the spleen. Dr. Sciamano of Rome first showed that blood may be thus obtained from the substance of the spleen during life without any injurious consequences. The blood of the general circulation was taken from the tip of the finger. In each method every precaution was taken to avoid the accidental introduction of organisms. The examination, in this way, of fifteen patients gave the following result. At the height of the disease the blood of the general circulation contains micro-organisms both isolated and grouped. These consist, almost exclusively, of spherical bodies, which have a delicate contour, appear to be homogeneous, and are analogous to micrococci. Some of them are mobile. Similar organisms, again, were seen in the blood of the spleen, and in it, too, were others, rod-shaped, also with delicate outlines, perfectly corresponding to those described by Eberth and Klebs. During convalescence these micro-organisms lessen in number in both the splenic and systemic blood. When quinine was given to the patient in large doses the organisms either disappeared from the blood, or were present in it only in small number. The blood from both the finger and the spleen was treated by the method of fractional culture, and a large number of rods were then obtained, similar to those seen in the fresh blood except that some of them were of greater length. The presence of such organisms in the blood of the spleen after death had been previously established by Sokoloff and Fischel, but Maragliano is the first who has demonstrated their presence in the splenic blood during life. He avoids the expression of any opinion as to their relation to the disease. (*Lancet*, Oct. 28, 1882.)

Diagnosis of Spurious Cardiac Murmurs.—Dr. Rosenbach recommends the following simple means of determining the nature of any murmur. Firm pressure is made with the tip of the finger in an intercostal space at the point of maximum intensity of the murmur. In this way the portion of lung in which the murmur is produced is pushed away from the heart, and the heart itself is also pressed back, and is prevented from transmitting its pulsations to the lung with force sufficient to give rise to a murmur. The spurious extra-cardiac murmurs are thus greatly weakened or entirely suppressed, while no effect is produced upon the true cardiac murmurs. (*Transactions of the German Medical Congress*, 1882.)

Reduction of Dislocations of the Thigh.—In cases where reduction of the femur by manipulation in the usual way, with the aid of anæsthetics, has failed, or is inapplicable, and as a substitute, in many cases, for anæsthesia, assistants, and mechanical power, Mr. Kelly proposes the following methods:

For posterior dislocations—The patient is laid prostrate upon the floor. Three strong screw-hooks are inserted into the flooring close to the perineum and each ilium of the patient, and to these hooks he is secured by strong bandages or rope. The injured thigh is flexed at right angles to the patient's body; the foot and lower extremity of the tibia are placed against the perineum of the surgeon, who, bending forward, with the knees slightly flexed, passes his forearms behind the patient's knee and grasps his own elbows. Reduction is now accomplished by drawing the femur upwards; but circumduction may also be practised; the surgeon, stepping backward, then extends the limb, and lays it by the side of its fellow. In sciatic dislocations in order to liberate the head of the bone from the foramen, a bandage may be passed around the thigh, close to the trochanter, by which an assistant may make traction.

For anterior dislocations.—The patient is placed upon a table of such elevation as to have his pelvis nearly as high as the trochanter of the surgeon. A bandage around the pelvis, and secured to the side of the table farthest from the dislocation affords counter-extension. The surgeon, with his face directed towards the dislocated joint, and standing on its inner side, with his trochanter pressed against the femur, now bends the leg behind his back, and grasps the ankle with the corresponding hand. Reduction is effected by rotating or turning his body partially away from the patient, thus making traction on the femur in the most favourable direction, and at the same time pressing its head towards the acetabulum with the disengaged hand. (*Dublin Journal of Medical Science*, Oct. 1882.)

Relation of Bacilli to Tuberculosis.—Dr. H. F. Formad,

Lecturer on Experimental Pathology in the University of Pennsylvania, has been conducting an elaborate series of researches on this subject, from which he draws the following deductions. We give them as a serious American criticism on Koch's doctrines. (1) The predisposition to tuberculosis in some men and animals, the so-called scrofulous habit, lies in the anatomy of the connective tissue of the individual, the peculiarity being a narrowness of the lymph-spaces, and their partial obliteration by cellular elements. (2) Only beings with such anomalous structure of connective tissue can have primary tuberculosis, and such animals invariably do become tuberculous from any injury resulting in inflammation, or from repeated injuries. (3) Scrofulous beings can have no other than a tuberculous inflammation, although it may remain local and harmless. (4) Non-scrofulous men or animals may acquire the predispostion to tuberculosis through malnutrition and confinement, the latter bringing on the above-mentioned anatomical peculiarities in the connective tissue. (5) No external ætiological influences are necessary to cause tuberculous disease other than those which ordinarily produce inflammation, and even scrofulous beings will not become tuberculous unless local inflammation is set up. No inflammation, no tuberculosis. (6) Non-scrofulous animals, so far as can be established now, may acquire tuberculous disease through injuries of serous membranes, viz., peritoneum, pleura, etc., and even then without any special virus whatsoever. Clinical observations on the post-mortem table show similar conditions and prove the same in man. (Dr. Formad also claims that Koch's own experiments are really in favour of this proposition; but that he has overlooked the inference.) (7) The bacilli, which it is the merit of Koch to have first proved to infest tissues affected by tuberculous disease, are not necessary for its causation, even if a special organism exist and be really possessed of such property. The presence of bacilli (so far as our present knowledge goes) is secondary, and appears to *condition* the complete destruction of the tissue already diseased and infested by them, and this destruction is in direct proportion to the quantity of the organisms, which thus regulate the prognosis. The tuberculous tissue seems to serve merely as a nidus for the growth of the bacillus. (8) From the results of microscopic examination, from numerous observations upon the post-mortem table, and on clinical grounds, he has come to the conclusion that phthisis is not a specific infectious disease, but that the individuals suffering from tuberculous disease are specific themselves originally, and form a special species of mankind, the "scrofulous." (9) Scrofulosis is a condition which may arise from malnutrition and seclusion in any being, and thus may be

produced artificially. It always depends upon the demonstrated anatomical changes in the connective tissue. (10) An analysis of Koch's experiments shows that he has not proved the parasitic nature of phthisis, or that there exists a special *Bacillus tuberculosis;* so that the infectiousness of tuberculous disease is still *sub judice.* (*Philadelphia Medical Times,* Nov. 18, 1882.)

Transfusion and Ether Injections.—In a paper read at the Académie de Médecine, Prof. Hayem related the results of his experiments in controversion of the accuracy of Prof. Verneuil's statement that transfusion is a useless operation, which may be superseded by hypodermic injections of ether. Having bled a dog almost to the point of death, he found that the injection of ether was attended with no durable effect, while transfusion produced a "true resurrection." When a large quantity of blood was withdrawn, the dilution of what remained by the transfer of the serum derived from another dog was also attended with the same success. The stimulation by ether only produces an increase in the energy of the cardiac contractions and a notable increase in the number of the pulsations, but does not give rise to any increase of the pressure of the blood nor of the rectal temperature. (*Gaz. des Hôp.,* Dec. 21, 1882.)

Ligature of the Bile-duct.—Beloussow has studied this subject under the direction of Cohnheim and Weigert. He experimented upon rabbits, guinea-pigs, and dogs. The longest time that any animal survived was eighteen days. The liver was jaundiced and slightly enlarged. In its substance were seen yellowish gray spots varying from the size of a pin's head to a pea. These were most numerous from the first to the sixth day. The microscopic examination showed them to represent a partial necrosis of the liver substance caused by the pressure of the bile. Around these nodules appeared a zone of reactive inflammation with the formation of young connective tissue in which were newly-formed bile-ducts. This new tissue gradually replaced the necrotic portions entirely. In this way is to be explained the cirrhosis of the liver observed by earlier experimenters (Wickham, Legg, Charcot, Gombault, and others) after the ligature of the ductus choledochus. This occurred in entirely aseptic cases, and was in no way to be connected with any inflammation starting from the point of ligature and following up the course of the bile-ducts. Kelsch (*Rev. de Méd.,* 1881), records two cases where the retention of bile was followed by cirrhosis,—one following closure of the duct by cholelithiasis and cancer of the gall-bladder, the other in which a dilatation of the

bile-ducts was found without any formation of concretions. (*Arch. f. exp. Path.* vol. xiv.)

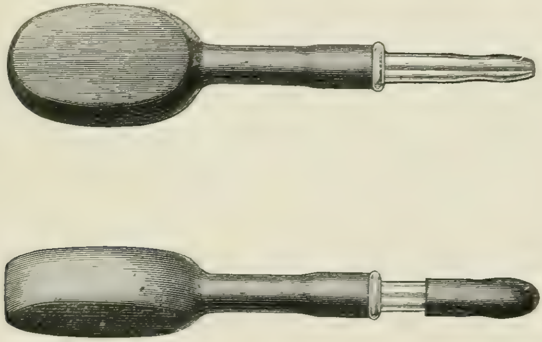
Test for Iodine in the Body.—Dr. Henry A. Lediard, of Carlisle, records that a patient taking twenty grains of iodide of potassium three times a day, was asked to spit into a wineglass. A little calomel was added, and soon a canary-yellow coloration appeared.—Another patient, with a patch of ulceration, taking iodide of potassium as in the first case, was treated by dusting calomel over the sore, and the yellow colour was obtained.—Another patient, taking one-sixteenth of a grain of the bichloride of mercury with five grains of the iodide of potassium thrice daily, had the tonsils dusted, by means of an insufflator, with calomel; the yellow colour was immediately present. The case was one of syphilitic affection of the mucous membrane of the mouth and fauces, and the same phenomenon was observed when his saliva was mixed with calomel upon a slide of glass.—A woman with syphilis, taking a drachm of iodide of potassium daily, presented herself. Testing the saliva, it gave no reaction. She was charged with omitting to take her medicine, and owned that none had been taken for five days, because her bottle was finished; hence the test may be used to ascertain the absence as well as the presence of iodine in the body.—Another patient had taken the same morning two doses only of five grains each of iodide of potassium, and she also gave the reaction in the afternoon, showing how rapidly-absorbed a drug is this salt of potassium. —A still more striking example of this rapid absorption was given by a gentleman who was threatened with a gouty attack. Five-grain doses of iodide of potassium were prescribed together with a little bicarbonate of potash, colchicum, and decoction of broom. As he was very susceptible to the action of iodide of potassium, he was asked to test the saliva after each dose of the medicine, until he obtained a yellow colour with calomel; with this result, that, four hours after one dose only, the yellow colour was obtained, together with tenderness under the jaws, and some salivation. This case shows how delicate a test we now have for the presence of iodine in the body.—The test is, further, unaltered by the addition of carbonate of ammonia. Iodide of potassium is as readily eliminated as it is easily absorbed; for, when the internal administration ceases, in a very few hours the test is no longer obtained; and this holds good even when so large doses as a drachm of the iodide are being taken daily. (*British Medical Journal,* Nov. 18, 1882).

The Cure of Strabismus by the Employment of Glasses.—M. Boucheron recommends the use of corrective

glasses in cases of convergent strabismus, an affection that is usually associated with hypermetropia or long-sightedness. The employment of appropriate corrective glasses must, however, be preceded, as Green recommended long ago, by the instillation of atropine to paralyse the accommodation, and the effect of the atropine should be continued for two or three months, if the case is seen at the outset; or for ten or twelve months if the affection has lasted some time. In fact, the employment of the atropine should be continued as long as there is any tendency to strabismus. The glasses ordered should be of sufficient strength to neutralise completely the hypermetropia present. When no further tendency to strabismus is observed, the atropine may be discontinued, but the glasses must still be employed. The strength of the atropine solution he uses is one part of the sulphate to 300 of water. If atropine produces any irritation, he uses duboisine. When strabismus is fairly established he resorts to tenotomy, but even after this is performed he instils atropine. (*Annales d'Oculistique*, Dec. 1882.)

Notes and Queries.

Mr. B. Squire's Urethral Syringe.—This is a simple and cheap instrument, made of glass and vulcanised india-rubber, as shown in the woodcut. It is difficult to see how it can get out of order so long as the glass remains unbroken. The advantages claimed for it are these :—It is easily worked with one hand; the other hand being left free to close the mouth of the urethra around the nozzle. It can be *completely* emptied (by pressing the sides together), and *completely* filled (by relaxing the pressure). Moreover, its fluid capacity is precisely that which is necessary to distend fully, but not unduly, the male urethra with fluid. Consequently no air-bubbles can be injected



(Two-thirds nat. size.)

by it into the urethra, and no fluid into the bladder. Its flat shape and small size enable it to be carried easily in the waistcoat pocket. A supply of liquid, enough for one injection, may be carried in the syringe. The nozzle is provided with a rubber cap which takes off and on; so that the syringe, filled with a supply of solution, may be carried safely in the waistcoat pocket. The syringe is well finished, and is manufactured by Messrs. Ingram, at the London India-rubber Works.

Reports of Local Government Board.—For the convenience of members of the medical profession and others, copies of certain of the Reports made to the Board by their Medical Inspectors will henceforth be placed on sale, and can be purchased from Knight and Co., 90 *Fleet Street ;* Shaw and Sons, *Fetter Lane ;* Hadden, Best and Co., 227 *Strand ;* P. S. King, *King Street, Westminster.*

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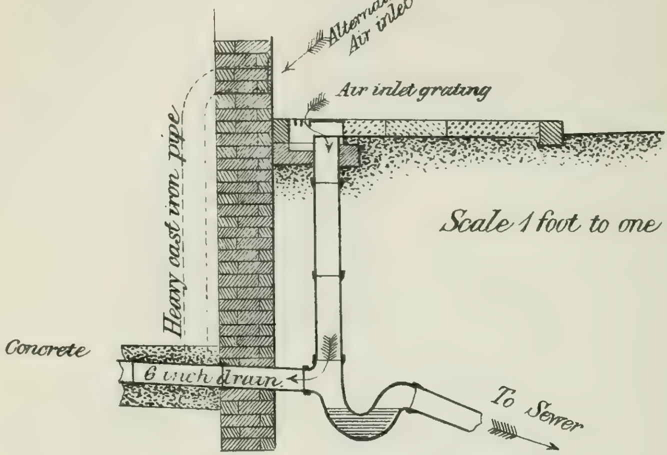
The Essentials of Bandaging; with Directions for Managing Fractures and Dislocations . . . and containing a Chapter on Surgical Landmarks. By Berkeley Hill, M.B., F.R.C.S., Professor of Clinical Surgery in University College, London, &c. Fifth Edition. Post 8vo, pp. 341. Illustrated. 5*s.* London: Smith, Elder, and Co. 1883.

The Systematic Treatment of Nerve-Prostration and Hysteria. By W. S. Playfair, M.D., F.R.C.P., Professor of Obstetric Medicine in King's College, London, &c. Post 8vo, pp. 92. 2*s.* 6*d.* London: Smith, Elder, and Co. 1883.

*** Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams and Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

DETAIL OF MAIN DRAIN DISCONNECTING TRAP
WITH AIR INLET IN STREET FOOTPATH.

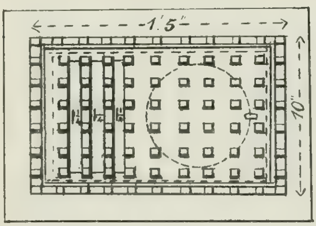


Scale 1 foot to one inch.

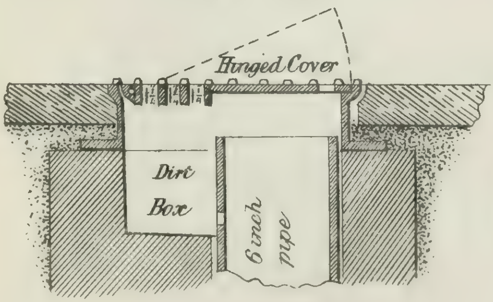
GENERAL SECTION

DETAIL OF AIR INLET
IN FOOTPATH

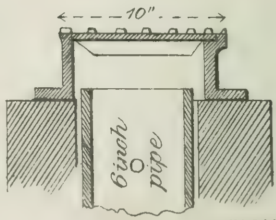
Scale 1 foot to one inch.



PLAN OF COVER



SECTION



CROSS SECTION

Rogers Field M.Inst.C.E.

Department of Public Health.

HEALTH AND ECONOMY IN THE UNITED STATES.

The National Board of Health for the United States has, since its creation, performed its duties in such a way as to meet with a verdict of approval not only on the American continent, but in all parts of the world; it has succeeded in creating an interest in matters relating to public health in quarters where the subject had heretofore passed all but unnoticed; and by its co-operation with medical, scientific, and sanitary associations both at home and abroad, it has identified the population which it represents with sanitary progress in both hemispheres. Under these circumstances it is humiliating to learn the estimation in which the Board is held by some members of the State. Under an Act passed in 1879 the Board was required to obtain information concerning the health of foreign ports, and to publish and transmit to certain officials and others, weekly abstracts of the information received. This was effected by means of the Bulletin of the Board which was issued from the central office to persons interested in the maintenance of health in the States; and it was of special value as affording a medium of communication between a large number of health officers and their respective boards. But in July last year, the issue of this little, but valuable, pamphlet was suddenly suspended, and the sanitary world became aware of the fact that this course was necessitated by the failure of Congress to find means for its support. It was much hoped that such a result had been caused by some accidental oversight in the control of the public finances, and that it would be at once rectified; but instead of

this we learn further, that, for a similar reason, a large amount of information which is of the highest importance in connection with the maintenance of public health, is either lying idle, or remains in an unfinished state, in some pigeon-holes in Washington. The last Annual Report of the Board tells us of reports in their possession which deal with subjects of the greatest interest, but which cannot be published for want of funds, and of investigations giving promise of the best results which, under the same circumstances, have suddenly been arrested.

To refer to one such matter only. There are few subjects of much greater interest to the public, than the determination of the real value which should be attached to the chemical analysis of water, and of the specific effects produced on health by the use of contaminated waters. To this subject therefore the Board directed its special attention. An elaborate system of inquiry was set on foot, the independent services of a number of skilled chemists and microscopists were secured, and finally the results obtained were submitted to the Board, who now report that with the small fund available for printing, they are unable to publish them.

And even this is not the worst. The very existence of the National Board is assailed, and this mainly on the ground of alleged extravagance in the administering of its funds; an accusation which hardly stands the test of the most cursory examination. One ground adduced for its abolition has reference to the fact that it was established with special regard to cholera and yellow fever, and the conditions which led to its creation, having ceased, it is alleged that it also ought, *ipso facto*, to exist no longer. We would however call attention to the fact, that not only in this the mother-country, but in nearly all European states, the existing State Boards of Health, and indeed nearly all sanitary legislation, have had a very similar origin; an epidemic prevalence being either anticipated or in actual existence at the time. In almost every country there are some who are unable to see the value of a central health authority which acquires information at all hands, and makes the experience thus obtained available throughout the area of its jurisdiction; and amongst ourselves there are some so-called

statesmen who would gladly deprive the State they profess to represent, of the benefit which accrues from scientific investigations conducted at the public cost. Happily, however, they are but few in number. And since we can hardly conceive of their being in any noteworthy degree representative of public opinion on the other side of the Atlantic, we would urge upon all who really value the well-being of the State, not so much to maintain the National Board, as greatly to extend its usefulness, by enlarging the powers under which it has already done such good work, in the improvement and preservation of the public health.

IMMIGRANT INSPECTION IN THE UNITED STATES.

At a time when the sanitary aspects of immigration are engaging the attention of our own government, special interest attaches to the record of any action taken in a similar direction by other governments. During the year 1881 small-pox began to spread to an alarming extent in the western and north-western States of America, the disease being introduced by immigrants coming from ports where the disease was known to exist. As a rule the poison had been received a day or two before sailing, and since the voyage, in nearly all cases, lasted less time than the ordinary incubation period of fourteen days, the persons affected exhibited no symptoms until after they had landed and had commenced their journey westward. Chicago and several other centres were thus infected, and hence in anticipation of the usual spring and summer immigration of last year, the National Board of Health decided to take special action with regard to passengers coming to the United States from any foreign port where small-pox prevailed. The consular agents at the ports of departure were communicated with, and their action led to active measures for the vaccination of all steerage passengers before, or soon after, embarkation. A special system of immigrant inspection was also established in the

United States, both at every port of entry and at the principal railway centres in the interior, the main object of the inspection being to ascertain if any of the immigrants either suffered from small-pox or were without such protection against that disease as is afforded by efficient vaccination, and the whole system was under the general supervision of Dr. Stephen Smith, a member of the National Board. Starting on his journey west the immigrant is examined, and if found to be properly vaccinated, or if then submitting to the operation, he is furnished with a "protection card," which holds good until he reaches the next inspecting station, where he is again examined in order to see whether any vaccination previously performed has succeeded or not; the process being repeated until the final destination or the frontier of the States is reached. If on the route a case of small-pox is discovered, the patient is isolated in a quarantine-car, and at the next "refuge station" he is removed, his fellow passengers in the car where he was travelling, together with their effects, undergoing a system of cleansing and disinfection. All the unvaccinated, or badly vaccinated, are offered gratuitous vaccination; the necessary inspections and operations being conducted, when necessary with a view of preventing delay, by a travelling inspector whilst the train is in motion. Freedom in the matter of vaccination being the rule in the United States, there is, to quote the Annual Report of the National Board of Health, "in all this no compulsion," but we are significantly informed that every person not protected by vaccination is told that the health authorities beyond the State line will probably refuse him admission within their jurisdiction unless he submits to the operation.

The whole system, including the indirect compulsion so wisely resorted to, has worked well along all the lines of immigrant transportation, and in localities where heretofore small-pox had prevailed in an epidemic form, the disease has practically been eradicated. Thus, the Secretary to the Illinois State Board of Health, writing as regards a state through which fully 1,500 immigrants were passing every day, and having as yet had only seven weeks benefit from the inspection system, says that for "the first time in eighteen months small-pox is practically under control," there having been only a single importation

of the disease since the inspection service commenced. Whether the service is to be permanently retained or not remains to be seen. As yet the funds for its maintenance are not provided, and no decision has been arrived at on the subject, but it is to be hoped that for the benefit of the many millions through whose territory the immigrants pass, and in the interests both of the inhabitants of the localities in which they settle, and of the immigrants themselves, a system which has already produced such good results will not only be continued, but will be extended in such directions as may hereafter be found expedient.

ANNOTATED MODEL BYE-LAWS OF THE LOCAL GOVERNMENT BOARD.

BYE-LAWS exercise a more important influence on the future of the districts to which they are applicable than is commonly supposed, and those which relate to matters of construction are specially calculated to produce a result which, if not permanent, is at least certain to remain in operation for a considerable period of years. Whether this influence is to be for good or for evil depends upon the nature of the bye-laws, and on the stringency with which they are carried out.

Bye-laws are designed to supplement certain provisions of the statute law, and they relate to so large a number of subjects that we cannot profess to refer, even in general terms, to more than a few of them in the present article. Several of the subjects have, however, a direct bearing on questions affecting public health, and there are probably few, if any, urban sanitary districts in the kingdom where some bye-laws are not in operation, as to such questions as nuisances, the keeping of animals, the construction of new buildings, and, to a less general extent, to the cleansing and removal of refuse from premises.

Unfortunately, however, many of the bye-laws dealing with these and kindred subjects are based on altogether wrong principles, and hence they are producing a result which is

distinctly injurious to the interests of health in the districts where they are in operation. Thus, the abominable midden-privy system is, in many of our northern towns and districts being perpetuated owing to bye-laws under which the construction of any improved form of closet is prohibited under penalty. The principles which should govern the construction of such structures have been distinctly laid down, and they may be briefly summarised as involving (1) dryness of contents; (2) the thorough mingling of ashes with the excreta; and (3) frequent removal of contents. But bye-laws are widely in use which enforce the violation of all these principles. Middens open to rainfall, and sunk below the level of the surrounding ground so as to ensure the soakage into them both of surface and sub-soil water, are constantly being erected under bye-laws, framed as if purposely to prevent all chance of maintaining dry contents. The capacity of middens, instead of being limited to some small maximum, is only regulated by the definition of some minimum, specially designed to secure the retention in close proximity to dwellings of such huge quantities of excremental and other filth, as will make it rarely necessary to resort to the process of scavenging. The midden too is so placed with regard to the privy as to render all but impossible, that due mingling of ashes and dry refuse which is essential to the prevention of nuisance. In regard to new buildings, faulty regulations are in a similar way enforced under bye-law, although owing to the vague manner in which the clauses are worded, it is at times difficult to specify any one point which is mischievous. There are, however, codes of bye-laws which require, for example, that rain-pipes shall act as drain ventilators, a requirement which has often led not only to nuisance, but also to injury to health, and which besides fails properly to secure the end held in view. Thus, rain-pipes as a rule have their heads near bed-room windows, and many are situated beneath the overhanging eaves, conditions which go far to insure the entrance into dwellings of the drain air as it rises up the pipes, and which should have sufficed to procure a prohibition of the use of such pipes for the purposes of drain ventilation. And further, the time when the current of drain-air up a ventilator should be most free, is that when sewers and drains are most rapidly filling, as for example, during heavy rainfall;

this however is precisely the time when the rain-pipes are occupied by a down current of water, which effectually hinders and impedes any attempt of the drain air to rise up the shaft, at the top of which it is expected to make its exit.

The vague character of many building bye-laws has also operated quite as injuriously as the direct imposition of regulations which are mischievous in themselves. It is a common thing to find a large number of bye-laws which indicate in very general terms what is required, and then end by saying that the work in question must be performed so as to meet with "the approval of the Local Board" or, "in such manner as the Local Surveyor shall direct;" conditions which enable the "jerry-builder" to have his own way with his fellows on the Local Board, and which conduce to bribery and fraudulent transactions in dealing with certain surveyors, many of whom are utterly incompetent, being, indeed, often mere road labourers or ill-educated inspectors of nuisances. Then again, it is a common practice to lay down certain requirements, and in the next sentence to declare that "they may be modified in certain cases at the discretion of the Local Board," or to assert that they must not be deviated from "without the approval of the Local Board." In this way there has resulted an uncertainty as to the operation of bye-laws which has in many instances made them even worse than useless; and persons having either to comply with them or to secure the modifications which they desire, have found themselves entirely at the mercy of some capricious officer of the authority.

Bye-laws of the character we have described, were some few years ago, all but general throughout England and Wales, and they were to a large extent based on certain forms which were issued by the Local Government Act Office, then under the direction of the Secretary of State for the Home Department. After the formation, however, of the Local Government Board, with its several departments dealing with sanitary matters, it was recognised that the bye-laws in force were very generally of an extremely faulty character, and the result was that that Board in 1877 commenced the issue of a series of model bye-laws, which has only recently been completed by the publication of a code regulating noxious trades. The need for

some standard series was the more urgent, because under the influence of the Public Health Act, 1875, a large number of sanitary authorities, rural as well as urban, were desirous of providing themselves with bye-laws on several subjects, and because under the same Act rural authorities, hitherto without means of controlling building operations, were enabled, under an order of the central authority, to acquire urban powers with regard to such matters.

In compiling the model bye-laws, considerable pains were evidently taken to avoid the errors of the series which emanated from the Local Government Act Office. The bye-laws were framed so as to be in harmony with the statute law of England; they were couched in language which was certain, definite, and free from ambiguity; no clauses were so worded as to give to sanitary authorities the power of suspending their operation, unless in certain exceptional instances where the conditions under which such a power might reasonably be exercised could be specifically defined; and further, where rules which were enforceable by penalties were laid down, the necessary details as to the methods of complying with them were distinctly specified. So also, an effort was made to secure that every bye-law should be reasonable; and no clauses were inserted which either attempted to vary or to supersede the express provisions of the law, or which aimed at any alteration or amendment of any enactment in force. Some bye-laws which had heretofore been embodied in many of the codes in use, and which had operated beneficially, hence came to be excluded. Thus, one which regulated the height of living rooms, and required, subject to some modification as regards rooms in the roof, that all such rooms should "in every part be eight feet in height at the least from the floor to the ceiling," had to be abandoned as not coming within the range of subjects as to which bye-laws were permissible under the Public Health Act.

Some five years have now elapsed since the principal series of these model bye-laws were issued, and the extent to which they have been resorted to in framing codes for sanitary districts in various parts of the country, has afforded abundant proof of the estimation in which they are held. They have, however, not passed without criticism, and from some of the comments which

have been made as to them in papers read before certain societies, it is evident that points which might usefully have been included in them, do not form part of the code as originally issued. But many of the objections which have been made as to them have been founded on misapprehension. Thus, it has been very generally assumed that they were issued for adoption by all sanitary authorities, and in all sanitary districts alike, without any reference to local conditions which ought to be held in view; whereas it is quite clear from the manner in which they have been submitted to the various sanitary authorities that such modifications as were required to suit the particular circumstances of different districts were not only anticipated but distinctly invited. In short, the bye-laws were intended in the main to serve as models in the compilation of codes for the various sanitary districts requiring such local regulations.

It is true, that in some respects deviation from the model clauses has been either altogether refused, or has been assented to with considerable reluctance, but under these circumstances some important principle is generally in question, and even Sir Charles Dilke, the newly appointed President of the Local Government Board, who can certainly not be accused of any desire to sanction unnecessary interference on the part of a central authority with the wishes of local bodies, has recently explained that since bye-laws, when officially approved, can be enforced by penalty, they must necessarily be subjected to revision, and such revision should obviously free them from the risk of compelling persons to carry out works which are wrong in principle, and which may lead to the very results which they have been framed to avoid.

The precise and definite language which it became necessary to adopt in the compilation of the model bye-laws, and the somewhat lengthy phraseology which, for legal considerations, was needed in order to secure that each clause should really fulfil its intended purpose, have, however, tended materially to limit the extent of their adoption. Many sanitary authorities have been deterred from considering them, by reason of the necessarily complex and detailed nature of many of the clauses, and they have regarded many of the bye-laws, the purport of which they failed to understand, as needlessly stringent, and as

quite inapplicable to their districts. As a matter of fact, however, some of the bye-laws involving nothing which can be regarded as abstruse or complex in their application, are, by reason of their being framed so as to meet the many varieties of locality and of building, somewhat obscure to the unskilled and non-technical reader, and it must be admitted that some trustworthy explanation both of the scope and precise meaning of many of the clauses, would have removed much of the hesitation which has been exhibited on the part of sanitary authorities as to their adoption. We would hence draw special attention to the recent issue by Messrs. Knight and Co., Official Publishers to the Local Government Board, of an annotated edition of the three series of model bye-laws which are most in demand.[1] The bye-laws dealt with in this work are those that relate, 1st, to the cleansing of footways, ashpits, privies, &c., and to the removal of house refuse; 2nd, to the prevention of nuisances and the keeping of animals; and 3rd, to new streets and buildings. To each of the numerous clauses included in these three series is appended a note explaining its object, the grounds which render its adoption necessary, and the method of its application. Where the regulations admit of modification, the points as to which deviation from the model clauses may reasonably be entertained are indicated, and in a large number of instances diagrams have been prepared with a view of further illustrating the intention of the several clauses. With a view of making the work as complete as possible, the publishers have included in the volume the several letters which were addressed by the Local Government Board to sanitary authorities when the model bye-laws were issued, and in the form of an appendix they have brought together information as to the amount of excreta and refuse which have to be dealt with weekly, monthly, and quarterly in ordinary households, as also the text of the clauses of the Public Health Act, and other Acts bearing upon the question of bye-laws and their operation. Several modifications of the bye-laws as originally issued, and several additional bye-laws which either local custom or conditions special to certain

[1] *Knight's Annotated Model Bye-laws of the Local Government Board, with Diagrams and Approved Additional Clauses.* London: Knight and Co., Local Government Publishers, 90, Fleet Street, E.C.

districts, have rendered necessary, are also introduced; and a considerable amount of matter bearing upon the principles which should govern local authorities in their sanitary administration, is embodied in some of the explanatory annotations. Some of the latter we may usefully refer to in detail.

Taking, in the first instance, the series relating to the removal of excremental and other refuse from the neighbourhood of dwellings, it is at the outset pointed out that before making bye-laws at all as to this subject-matter, the sanitary authority should consider whether it is desirable to impose such a duty on occupiers, and whether the work cannot be far more effectually carried out by the authority themselves. Section 42 of the Public Health Act, 1875, enacts that every local authority, whether urban or rural, may, and when required by the Local Government Board shall, themselves undertake or contract for such work, and it is quite obvious that, except in sparsely populated districts, where every occupier has a suitable piece of garden ground, the removal of such refuse, at intervals such as are necessary in the interests of health, cannot be effected except when the work is in the hands of the authority. Indeed, the practice of casting the duty upon the occupier has indirectly been the cause of serious nuisance, of injury to health, and of much preventable death. The difficulty of procuring persons to carry out the scavenging, especially during harvest time when farmers cannot spare their labourers to fetch away the refuse, has necessarily led to the construction of receptacles capable of containing needlessly large accumulations, and the receptacles being large they are allowed to fill before they are emptied. In this way it has come to pass that householders have very generally acquired the habit of storing up in their back yards or other premises a huge quantity of refuse, often in a state of advanced decomposition and tainting the atmosphere with its filthy emanations. Where human excreta have formed a part of the refuse, the danger has been especially great, and many a prevalence of enteric fever and of infantile diarrhœa has owed much of its spread to such conditions. Indeed, in making bye-laws, it should be a principal aim of authorities to limit as far as practicable the capacity of all the filth receptacles, and this can, as a rule, only be effected when

they themselves arrange for the emptying of such receptacles at the needed frequent intervals.

Amongst the clauses in the series as to the keeping of animals, is one regulating the position of piggeries with regard to houses and sources of water which are likely to be used for any domestic or allied purposes. The distance at which such structures should be placed from dwellings and water-sources is not specified, it being left to the authorities to propose a distance adapted to the circumstances of their districts. But in many localities where some such clause is in operation, the distance specified is so small that the bye-law, instead of being one for the "prevention of nuisance," ought rather to be regarded as one which brands with official sanction a condition which is calculated to create nuisance. Under these circumstances we are glad to find that in the annotation following the model clause, a case is quoted in which the Court of Queen's Bench have held a bye-law prescribing a minimum distance of 100 feet to be "reasonable," and it is to be hoped that this distance may, under ordinary circumstances, come to be regarded as the smallest minimum permissible. In districts where, owing to the overcrowding of houses on space, a reasonable distance cannot be procured, the remedy should not be to permit of a distance which will not secure freedom from nuisance, but rather to insist on one which will prohibit the keeping of pigs altogether.

The series relating to new streets and buildings is perhaps the most important of all; it is certainly the one which contains the largest number of clauses. Many of these relate, however, to matters which either do not affect health at all, or else only do so in a very indirect way; and concerning these we would only say that the explanatory notes and diagrams have evidently been prepared in such a way that they must necessarily be helpful in the extreme both to architects, builders, and others concerned in structural works, and also to those officers of the sanitary authorities whose duty it is to see that such works are carried out in compliance with the bye-laws.

Selecting a few clauses which deal with health considerations, we note, first, the bye-law which requires the removal of all filth from sites before the foundations of dwellings are laid.

There is a general impression that as regards some deposits of refuse, lapse of time alone suffices to remove all injurious matter, and in connection with this subject, the results of some experiments made by Dr. Burdon Sanderson, F.R.S., and the late Dr. Parkes, F.R.S., as to the effect of time on organic matters which, together with cinder refuse, had been used to fill up inequalities of ground in the borough of Liverpool, are quoted. From these it would appear that the process of decay of all the most easily destructible matters, including vegetable refuse, is completed in three years, and that it is virtually innocuous before that time. In the case of wood and woollen cloth the process is more prolonged, and whenever fœcal matter has formed part of the deposit, a decidedly longer period should be allowed to elapse before building operations on any such site can be sanctioned. Another clause requires that the sites of new domestic buildings shall be wholly covered with concrete. It is very generally assumed that this precaution is only necessary with a view to prevent the passage of dampness from the soil into the houses above, and that it has concern in the main, with the prevention of pulmonary consumption. But important as this consideration is, the annotation appended to this bye-law points out that other injurious influences have to be contended with. In the first place, we are only just beginning to understand that, under certain circumstances, ground-air has a distinctly prejudicial influence on health, and that the more porous soils, which are generally regarded as the best for the purposes of foundations, hold in their pores large volumes of ground-air, which must necessarily be drawn into dwellings unless excluded by some such process as that of concreting the house site. So also, when foundations are laid in specially dry and pervious soils, as, for example, in chalk, rock, or in gravel, serious and fatal disease has often resulted from the emanations resulting from a fouling of the soil by reason of leakage from some neighbouring drain or cesspool, a result which is entirely obviated by the simple process embodied in the bye-law, one, too, which, as pointed out in the annotation, is the reverse of costly, seeing that a cubic yard of concrete only costs from ten to fifteen shillings, and, at six inches thick, will cover an area of fifty-four square feet.

Passing by a large number of other clauses, we note next those which relate to the question of house-drainage, and which are both fully explained and somewhat copiously illustrated by means of wood-cuts and lithographs. The clause which enacts that every house-drain shall be so constructed as to admit of a current of fresh air through its entire length, is one of the most important ever included in a code of bye-laws. The details into which the clause enters with a view to its application to all houses, however they may be situated with regard either to each other or to the adjoining public road, necessarily make it a somewhat lengthy one, and to the ordinary reader it is not at once apparent how its provisions may best be carried out. In the work published by Messrs. Knight & Co., this clause has received considerable attention. Every single paragraph is separately explained both in the text and by reference to diagrams, and a series of lithographic drawings have been specially prepared by Mr. Rogers Field, C.E., to illustrate how the various regulations as to house-drainage may, under varying circumstances of site and position, be most efficiently carried out. One plate shows, by means of a block plan and an elevation, the house-drain arrangements in the case of houses that are completely detached, a second deals similarly with houses that are semi-detached, and a third with houses that form a continuous row. Each plate indicates the trap which shuts off the sewer-air from the house-drain, the two openings in the course of the house-drain which are necessary to secure its through-ventilation, and the various methods in which the ventilating shafts may be erected. Then follow other plates showing the detailed construction of the inlet-shafts for fresh air, and some of these plates are specially useful because they point out clearly how the arrangements for through-ventilation of drains may be effected in case of houses abutting immediately on the public thoroughfare, and as to which it has often been assumed that the provisions of this bye-law could not apply. By permission of the publishers we reproduce one of the lithographs dealing with this latter class of house. In the instance shown, the drain, passing beneath the house, is required to be embedded in and surrounded with concrete, and in its passage to the sewer it is provided with a disconnecting trap having on

the side nearest to the house a ventilating shaft for the admission of fresh air. The position of this shaft may vary according to circumstances. At times it constitutes the longer of the two required ventilating shafts, and is carried up the front of the house. But it is often desirable to arrange it otherwise, and two of the alternative methods of carrying it out are indicated in the annexed diagram. One arrangement shows the ventilating shaft opening out on the surface of the pavement, a plan which has very considerable advantages, but which cannot always be adopted, because it needs the sanction of the local authority, a condition which equally applies when the air-inlet shaft is made to open in the side of the kerb, as shown in a separate plate. Should this permission not be obtained, the same result may be obtained by placing a shaft, consisting of heavy cast-iron piping, well caulked at the joints, either in a chase in the wall of the building or immediately against the inner face of the wall, with a bend at the top leading to an opening in the outer face of the wall just above the ground-level, as shown by means of the dotted lines. Where the opening is placed in the surface of the pavement, it should be covered with a properly-fitting cast-iron lid, duly perforated, and be fitted, immediately beneath the apertures for the entrance of air, with a small dirt-box provided with a "weeper," through which rain-water can make its escape. The details of such an air-inlet are shown in the lower part of the diagram which we reproduce.

Amongst the other diagrams prepared by Mr. Rogers Field is one showing how waste-pipes from baths, sinks, &c., and rain-pipes should be disconnected from the drains; and another deals with the disconnection of waste-pipes in the case of basements. This series of lithographic drawings as to house-drainage constitutes one of the most valuable features of the work, and their publication may be expected to have considerable influence in promoting the future healthiness of dwelling-houses.

One annotation deals with a matter we have already incidentally referred to and we read with regret, because it embodies an opinion of the law-officers of the Crown to the effect that there is no general power under Section 157 of the Public Health Act, 1875, enabling an authority to make bye-laws

regulating the height of rooms. The need for such a regulation in connection with the ventilation of habitable rooms is unquestionable, and we can only hope that the hint given by the law officers to the effect that "there may, however, be cases where reasonable regulations made with reference to the mode of ventilation would necessitate the rooms being of a certain height," may suffice to indicate to sanitary authorities and their officers the lines upon which some bye-law may be legally framed so as to secure this much needed end.

THE PRACTITIONER.

APRIL, 1883.

Original Communication.

REPORT TO THE ASSOCIATION FOR THE ADVANCEMENT OF MEDICINE BY RESEARCH ON THE RELATION OF MICRO-ORGANISMS TO TUBERCULOSIS.[1]

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WITHOUT entering into the history of the literature on tuberculosis, I must briefly indicate the researches which bear directly on the point at issue. In 1877 Professor Klebs published a paper[2] in which he stated that he had succeeded in cultivating an organism from tuberculous material with which he was able to produce tuberculosis, and he looked on this organism as the cause of the disease. He gave it the name of "Monas tuberculosum." His method of cultivation was what he termed "fractionirte Cultur," *i.e.* if a drop of infective fluid is mixed with 50 ccm. of a cultivating solution it is diluted 1,100 times, and if a drop of this is put again into 50 ccm. the original material is now diluted 1,110,000 times, and so on. In this way he made cultivations from artificial tuberculosis in white of egg, and found that the egg became turbid in two or three days from

[1] Handed in to the Association on February 1, 1883.

[2] *Prager Medicin. Wochenschrift,* Nos. 42 and 43. 1877.

the development of micrococci and short rods about 2 mm. in length. These rods possessed active movement, but it was the micrococci and not the rods which he looked on as the cause of the disease. He also found, on examining crushed tubercles, that there were numbers of dancing bodies, the movements of which were more than molecular, and he states that they are found in advance of the tubercle and cause the tubercle. He only mentions one experiment, where a small quantity of the cultivation (third generation) obtained from a guinea-pig inoculated with human tuberculosis was injected into the abdominal cavity of a cat. The animal was killed a month later, and the peritoneum was found to contain tubercular nodules.

Similar results were obtained in 1880 by Dr. Max Schüller.[1] He pounded a piece of human lung containing tubercles till it formed a thick liquid. This was then filtered, and the filtrate used to inoculate flasks containing Bergmann's solution. His cultivations were carried to the third generation, and he employed the fractional method introduced by Klebs. In twenty-four hours, as a rule, the fluid became turbid from the development in it of micrococci. The inoculation of these cultivations was followed in some instances by development of tubercle. It is most important to note that he was only successful as a rule with the first and second generation; the third was sometimes without result. He explains this on the view that the organisms become weakened in their infective power. Schüller went further than Klebs, and describes masses of micrococci in the tubercles, in the diseased synovial membranes of joints, in lupus, &c.

In 1881 Professor Toussaint published a paper[2] in which he states that he has cultivated from the blood of tuberculous animals and from tubercles an organism which is evidently a micrococcus, and that cultivations of this organism produce tuberculosis when injected into animals. As I shall have more to say about Toussaint's investigation in another part of this report I shall omit further mention of it here.

Aufrecht, in 1881,[3] stated that the centre of a tubercle does

[1] *Die skrophulöse und tuberculöse Gelenkleiden*, 1880.

[2] *Comptes Rendus*, 1881.

[3] *Pathologische Mittheilungen*, Magdeburg, 1881.

not consist, as is generally supposed, of broken-down cells, but contains micro-organisms, of which he has made out three kinds; very small delicate micrococci, similar micrococci united in twos and threes to short chains, and also shining, short, rod-shaped structures, very narrow and about half as long again as broad. These rod-shaped bodies are, according to him, of almost constant occurrence in tubercles. In this paper he does not state how they were demonstrated, but in a later paper he says that these bodies, which he regards as the same as Koch's bacilli, could be readily shown in sputum by staining it in a half per 1000 watery solution of fuchsin.

This brings us to the now famous research of Koch published in the *Berl. klin. Wochenschr.* No. 15, 1882. He showed that a peculiar kind of bacillus was constantly present in tubercle, natural or artificial, and in all animals, and he was able to cultivate this bacillus, and by inoculation with the cultivated organism to produce the same disease as follows the inoculation of tuberculous material.

About ten days after Koch published, Baumgarten wrote on a bacillus which he had found in artificial tuberculosis, and which was undoubtedly the same as that described by Dr. Koch. His observation was made quite independently of Koch's, but he did not furnish any proof that the bacillus was the cause of the disease.

In July and August last I visited Toulouse and Berlin for the purpose of seeing the methods and the work of those who have been chiefly engaged in these investigations.

I arrived at Toulouse on July 21st, and was very kindly received by Professor Toussaint, who showed me all his methods and the results of his work.

The materials which he employs as cultivating fluids are either an infusion of chicken or of rabbit, or blood-serum; the last is preferred. Blood-serum is obtained in the following manner:—A glass retort is perforated at the upper part for the reception of a cork through which a tube containing cotton-wool passes. The mouth of the retort is connected by means of a

piece of caoutchouc tubing with a smaller glass tube. The apparatus thus arranged is purified by washing with carbolic acid lotion and afterwards by boiling off some distilled water in it, the end of the small tube being sealed. When the apparatus is cool the end of the small tube is introduced into an artery of some large animal, a horse for example, the sealed end is broken and blood is allowed to flow in till the retort is half full. The end is then sealed, and when the serum has separated a purified pipette is passed through the hole at the top of the retort and a suitable quantity of the serum sucked up and put into smaller vessels. These vessels have necks of ground glass over which a cap fits closely, the upper part of the cap being drawn out in the form of a tube which is plugged with cotton-wool. They are purified by heat. When it is desired to examine the cultivation or to inoculate a fresh flask the following procedure is adopted:—The narrow end of a pipette is sealed, the upper part packed with cotton, and the whole purified by heat. The point is then broken off, the broken part heated in the flame, the cap of the flask rapidly lifted, a small quantity of the fluid sucked up into the pipette, the cap reapplied and another vessel inoculated or the material examined.

He examines the organisms in the fluid with a Vérick's No. 8 objective without a condenser and without staining them. He has not investigated the tissues with the view of finding the organisms which he describes. He showed me a specimen of the organisms with which he works, and they were undoubtedly micrococci chiefly in groups.

He generally cultivates from the blood of the heart of tuberculous animals. The skin is stripped off without any special precautions and the chest opened. Then with a heated knife a hole is made in the heart, and the heated end of one of the sealed pipettes introduced through this hole, the end broken off and some blood sucked up and put into the flask containing the cultivating liquid. The inoculated flasks are then placed in an incubator and kept at the temperature of 37° C. In a day or two they become muddy from the development in them of micrococci. He has made about fifty cultivations from tuberculous animals, and always finds the same micro-organisms.

He always, in the first instance, uses tuberculous material

from cows (*perlsucht*) to infect the animals with which he experiments; but in one case he produced tuberculosis in a rabbit with phthisical sputum, got precisely the same lesions, and on inoculation of the blood of this animal into serum he obtained precisely the same form of organism.

In his experiments on animals he always uses for purposes of injection of blood, cultivations, &c. an ordinary subcutaneous syringe. After using it he washes it out with 1–20 carbolic acid lotion and sometimes with alcohol as well, but he does not allow the carbolic acid to act for any length of time.

In injecting cultivations he generally introduces ten drops He has never used less than six or eight, but he thinks the quantity is a matter of indifference. He marks the animals by clipping their ears.

He showed me a series of specimens illustrating the production of tuberculosis by inoculation of tuberculous material from one animal to another. The disease produced was evidently the same as that obtained by all other observers by the inoculation of tuberculous material.

He finds that the injection of the micrococci cultivated in infusions very often fails to produce tuberculosis in rabbits, but the cultivations in serum are much more successful. I was shown three cases in which tuberculosis had been produced in this way. The cultivations take as long or even longer to produce tuberculosis than tuberculous material does (two or three months).

He has caused tuberculosis in animals by injection of the saliva, urine, and blood of tuberculous animals, and by vaccination from vaccine pustules developed on tuberculous calves.

He has not had any case of spontaneous tuberculosis in uninoculated animals kept separate from tuberculous ones.

Professor Toussaint gave me specimens of his micrococci, and also portions of various organs of tuberculous animals for examination. The result of my investigations will be given later.

While there I saw five post-mortem examinations on rabbits (besides one to be noticed later). These were :—

1. A rabbit which had eaten bread soaked with the blood and

juice of the muscle of another tuberculous rabbit. This was killed after the lapse of about two and a-half months. The submaxillary glands were enlarged and caseous, the lungs full of tubercles, also the kidneys, and a few nodules and ulcers in the small intestine. Spleen free.

2. A rabbit which had been vaccinated at the root of the ear with lymph from a tuberculous calf. The lungs, kidneys, and bronchial glands were full of tubercle. There was very slight alteration at the point of infection, but the glands at the angle of the jaw were enlarged. He says that this is so in all cases. The vaccination is done in the same way as in the case of children.

3. A rabbit into which had been injected six months previously three drops of blood from a tuberculous pig. Here there was no recent tubercle, only one or two nodules which may or may not have been tuberculous; nothing to show that the experiment had been successful. However he told me that two other rabbits which had been inoculated with the same blood at the same time had died some time previously with extensive tuberculosis.

4 and 5. Two rabbits inoculated with tuberculous material. Extensive tuberculosis of all the organs.

I called on Dr. Schüller, in Berlin, on August 5th, and had a long conversation with him on the subject of his experiments.

He admits that Koch's tubercle bacilli can cause tuberculosis. This he considers absolutely proved by Koch. But he thinks that other things may also be the carriers of contagion, and he still holds that it may have been transmitted by the micrococci described by him. He did not examine human tuberculous organs for micro-organisms, but in animals he found bodies which, in his opinion, were often without doubt micrococci. He did not have any specimens at hand, but he showed me several of his original drawings, which were of course similar to the figures in his book.

In reference to his views it is important to note that his cultivations did not always produce tuberculosis, that they did not always kill so rapidly as tuberculous material, and that phthisical sputum was more virulent.

I cannot sufficiently acknowledge Dr. Koch's kindness, nor

the readiness with which he placed the results of his experiments and his methods at my disposal. I spent ten days in his laboratory, and was able to appreciate his extreme care and accuracy in experimenting and his entire want of bias in drawing his conclusions. I can only indicate briefly some of the principal results of my visit.

Koch's method of cultivating on solid materials instead of in liquids is so well known that a reference to its advantages is unnecessary. In the case of the tubercle bacillus it was found that the organism would only grow at the temperature of the human body, and therefore it was necessary to use some other material than gelatinised infusions. He accordingly solidifies blood-serum at such a temperature as to leave the serum as transparent as possible. The serum is put into purified test-tubes plugged with cotton-wool, and for several days in succession (usually six) these tubes are kept at the temperature of 58° C. for an hour. At the end of that time the tubes are laid obliquely in the incubator so as to have a large surface for inoculation, and kept at the temperature of 65° C., or a little higher, till solidification is complete. This occurs in a few hours. The tubes are then kept for some days till it is certain that the serum has been thoroughly sterilised, and then they are ready for use. Koch generally employs sheep's serum.

In inoculating from animals immediately after death the skin is dissected back, and it is well to wet the skin with bichloride of mercury solution to prevent the hairs flying about. A large number of knives, forceps, and scissors are purified by heat and allowed to cool, and every fresh cut is made with a fresh instrument, which is immediately afterwards repurified by heat. The organ to be used for the experiment being exposed, tubercles are clipped out and introduced into the serum-tubes by platinum wires purified by heat, and then crushed out as much as possible. Tubes so inoculated are placed in the incubator, and if development occurs small opaque masses will be seen to appear at various points in about ten days. These masses gradually increase in size, and after a time are picked up and spread out on fresh serum, the fresh masses which form being again treated in the same way, and so on. By proceeding in this

manner all trace of original tuberculous material other than the bacilli is soon lost.

In inoculating from the lung of an animal which has died some hours previously (man or cow) the procedure is somewhat different. The lung is removed from the body and the surface washed with bichloride of mercury solution. A series of cuts are then made with pure knives throughout the organ till it may be supposed that a pure portion has been got, and then tubes are inoculated in the same way as before.

During my visit Dr. Koch showed me a large number of cultivations, among which may be mentioned cultivations from phthisical lungs, from acute tuberculosis in man, from scrofulous glands, from cheesy pneumonia, from perlsucht, from spontaneous tuberculosis of a monkey, from artificial tuberculosis of animals, &c. These all exhibited the same appearances of

FIG. 1.

growth and varied from the third and fourth to the sixteenth generation.

For purposes of injection of the cultivations into animals, special syringes are used. These consist of a glass tube, on each end of which the thread of a screw is cut and the metal fittings are thus screwed directly on to the glass. The piston is graduated, and the bulbous end is hollowed in the middle, and does not fill up the calibre of the tube (see Fig. 1). A sufficient amount of cotton-thread is wrapped around this end of the piston to make it fit the tube accurately. The washers are made of thin pieces of cork. After each injection the cotton-thread and the washers are removed and the syringe washed. Before use fresh thread and fresh washers are arranged, and the whole is purified by heat. In this way the syringe is thoroughly purified and there is no chance of contamination from its previous use. The process of inoculation is as follows :—

A tube of serum on which bacilli are growing being taken, the surface is scraped with a heated platinum wire and the masses of bacilli ground up in a purified mortar with a little boiled distilled water so as to break up the masses. This material is then injected.

On August 2nd, Dr. Koch inoculated a series of animals before me in the following manner with a cultivation of bacilli from a case of phthisis. This was the eighth cultivation, the first cultivation having been made directly from the lungs of the patient on January 24th, 1882:—

a. About half a syringeful of the cultivation was injected into the vein of the ear of a rabbit. This animal was killed on August 10th, and numerous tubercles were seen in the lungs. I brought back the organs of this animal with me, and will refer to them later.

b. About two drops of the fluid were injected into the anterior chamber of each eye of a rabbit, less being put into the left eye than into the right. On August 4th one could perceive minute white masses on the surface of the iris, more marked in the right than in the left. On August 10th both eyes showed well-marked tuberculosis of the iris. Dr. Koch writes me that he killed this animal on September 12th, and that there were numerous tubercles in the lungs.

c. A speck of the cultivation was introduced into the anterior chamber of the eye of a rabbit by means of a pure needle. On August 10th there was no very marked change perceptible. Dr. Koch killed this animal on September 12th, and found that there were numerous tubercles in the lungs. (Tuberculosis of the iris had previously developed.)

d. A syringeful of the cultivation was injected into the abdomen of a guinea pig, and a small bit was put under the skin of the abdomen. On August 10th there was a hard nodule at the seat of inoculation, and the nearest lymphatic glands were much enlarged. Dr. Koch writes that this animal died on September 5th, and was found to be extremely tuberculous.

While I was in Berlin I also saw post-mortem examinations of a number of animals which had been inoculated with cultivations of bacilli.

1. On August 1st a rabbit died which was inoculated on July 6th into the eyes with the sixteenth cultivation from a case of spontaneous tuberculosis of a monkey, the first cultivation having been made on October 14th, 1881. In the right eye the point of the syringe was introduced into the anterior chamber and the piston only just touched. In the left eye the point was introduced, but immediately withdrawn again, the piston not being touched. The right eye was seen to be completely converted into a cheesy mass and tubercles were present in the conjunctiva, especially at its reflection. The left eye still showed well-marked tuberculosis of the iris, with commencing caseation at the point of inoculation, and tubercles in the cornea. The glands below the jaw were enlarged and beset with greyish points. The lungs, liver, and spleen were full of minute tubercles. These were largest in the lung where there was commencing caseation at the centre of the nodules with hæmorrhages around the cheesy points.

2. Another rabbit inoculated on the same day with the same material, and in the same manner, was found dying, and was killed on August 1st. The post-mortem appearances were exactly the same as in rabbit No 1. Tubercles cut out of the lung, crushed between two cover glasses and stained by Ehrlich's method, showed numerous bacilli.

3. On August 2nd a third rabbit inoculated in the same manner, at the same time, and with the same material as Nos. 1 and 2, was found dead. The post-mortem appearances were essentially the same, but the tubercles were in the main limited to the lungs.

4. On August 5th a fourth rabbit of the same series was killed, and the post-mortem appearances were essentially the same. Tubercles of the lung crushed and examined were found to contain bacilli.

There were also other two post-mortems, but as I brought back the organs with me for examination I shall refer to them again.

To show me his method of cultivation, Dr. Koch inoculated several tubes of blood-serum as follows :—(*a*) Several little flat vessels and tubes were inoculated from a cultivation from the

mesentery of a guinea-pig inoculated with a cultivation from acute tuberculosis. This tube had been kept some time, and fungi were growing from the cotton-wool plug. As a result, of seven tubes three had fungi develop in them and in four the bacilli grew well. All the small flat vessels became impure, but nevertheless it could be seen that the tubercle bacilli were growing. (*b*) From rabbit No. 3 a number of tubes were inoculated from one lung. In this lung there was pneumonia as well as tubercles, and in all the tubes both tubercle bacilli and another form developed. (*c*) Several tubes were inoculated from tubercles from the lung of rabbit No. 4, but no development occurred. Koch thought that this was probably bad serum, for there were masses of crystals in it, and he has noticed that serum containing blood-crystals is not favourable for development.

I made sections of a variety of cases of tuberculosis, and Dr. Koch gave me several tubes containing bacilli and several pieces of tuberculous tissue for further examination. The result of my investigation of these will be narrated presently.

I received the following letter from Professor Klebs in reply to a request from me that he would state his present position with regard to the question of the organisms of tubercle. I will translate the part of his letter which refers to this subject. His letter is dated January 18th, 1883. He says :—

"The question as to the organisms of tubercle turns at the present moment on the decision of the preliminary question whether the bacilli described by Koch, those rod-like bodies which hold fast crystallised acid fuchsin even when subjected to the action of mineral acids, are to be regarded as the carriers of the virus of the disease. I must in the first place state, with regard to these bodies, that developmental processes have not as yet been demonstrated in them ; their multiplication on cultivations would not disprove their inorganic nature. Although I believe that they are organisms, the proof of it must yet be furnished by the demonstration of phenomena of life on their part, such as movement or division under the eye of the observer. I think it proper to leave this task to their discoverer. On the other hand, I can definitely state that even in the purest cultivations (Koch's own) there are always present finely granular masses which appear to possess the charactèrs of micrococci. The same bodies also appear in cultivations on microscopic slides. They may, since they are present in cultivations which are active, be the carriers of the virus just as much as the rods which can be stained, and to which alone Koch attributes that property. It is possible that these forms are different stages of development of the same organism, but it is also possible that the rods are of an inorganic nature, just as

one finds crystals in the form of rods in many cultivations, as for example, the characteristic albuminoid crystals in cultivations of monads on isinglass jelly.

"This question cannot be regarded as settled at present, it requires further investigations. Nevertheless, it seems to me certain that the development of tuberculosis by a virus capable of self-multiplication, *i.e.* an organism, as first asserted by me as a result of experiments in which active cultivations were obtained, has received an important confirmation by Koch's experiments. And further, the diagnostic meaning of Koch's rods seems to me to be of great value."

Professor Klebs ends by recommending the study of and the adoption of measures against tuberculosis in cattle, which is in his opinion "the most frequent source of human tuberculosis (scrotula)." [1]

[1] The following is the original of Prof. Klebs' letter:—

"ZURICH, *January* 18, 1883.

"DEAR SIR,—In answer to your request to express my opinion upon the particular forms of organisms associated with tuberculosis, I cannot give you but a very short abstract of an extensive publication that will be published in the *Arch. of Exp. Pathology.* But you will excuse me if I prefer to write in my language.

"Die Frage nach den Tuberkelorganismen wird sich im Augenblick um die Entscheidung der Vorfrage drehen, ob die Koch'schen Bacillen, also jene Stäbchen-förmigen Körper, welche crystallisirter, saures Fuchsin auch gegenüber der Einwirkung von Mineral-säuren festhalten, als die Träger des Kranheitsvirus zu betrachten sind. Ich muss zunächst bezüglich dieser Körper constatiren, dass Entwickelungsvorgänge an denselben bis jetzt nicht nachgewiesen sind. Ihre Vermehrung an den Culturen würde nicht ihre unorganische Natur ausschliessen. Obwohl ich glaube, dass es Organismen sind, müsste doch erst der Beweis geliefert werden indem man an denselben Lebens-erscheinungen nachweist, Bewegung oder Theilung unter der Augen des Beobachters. Ich halte es für zweckmässig diese Aufgabe dem Entdecker zu überlassen. Dagegen kann ich mit Bestimmtheit behaupten, dass auch in den reinsten Culturen (von Koch selbst) regelmässig feinkörnige Massen vorkommen, welche den Character von Micrococcen zu besitzen scheinen. Dieselben Bildungen treten auch bei Object-trägerculturen auf. Sie können, da sie in wirksamen Culturen vorhanden sind ebenso die Träger des Virus sein, wie die sich farbenden Stäbchen denen Koch ausschliesslich diese Function vindiciren will. Es ist möglich, dass beide Formen zu derselben Entwicklungsreihe gehören; es ist aber auch möglich, dass die Stäbchen Beimischungen unorganisirter Art sind, wie man solche in Gestalt von Crystallen in vielen Culturen antrifft, zum Beispiel die characterischen Eiweiss-crystalle in Culturen von Monadinen auf Hausenblasegallerte.

"Diese Frage kann vorläufig nicht als entschieden betrachtet-werden, sie erfordert weitere Studien. Dagegen erscheint es mir unzweifelhaft, dass die Erzeugung der Tuberculose durch die mit Vermehrungsfähigkeit ausgestatteter Virus, also einen Organismus, von mir zuerst auf Grund wirksamer Culturen behauptet, durch Koch's Versuche eine wesentliche Bestätigung und Begründung

Koch's original method of demonstrating the tubercle bacilli was to stain them with an alkaline solution of several of the aniline dyes. He recommended an alkaline solution of methylen blue prepared according to the following formula :—

Distilled water, 200 ccm.

Saturated alcoholic solution of methylen blue, 1 ccm.

Shake, and while shaking add of a 10 p. c. solution of caustic potash, ·2 ccm.

The specimens to be stained are left in this solution for twenty to twenty-four hours, or if kept at the temperature of 40° C. for half to one hour. Afterwards they are put in a saturated watery solution of vesuvin, which drives the blue out of everything but the tubercle bacilli, and as a result we have blue bacilli and brown surroundings. He states that other aniline dyes except brown, can be used in place of the methylen blue though they are not so good, that other alkalies can be used in place of potash, and that a more strongly alkaline solution, though it spoils the sections, stains bacilli which were not previously apparent. Hence he concludes that the essence of the matter is alkalinity of the stain. Other bacteria stained in this way become brown except the bacillus of leprosy.

Baumgarten's method of demonstrating the bacillus was quite different from Koch's. He had found that the mycelia of fungi occurring in sections of tissue, though intensely stained by aniline dyes, were concealed by the stained tissues, while on the other hand if everything was treated with dilute alkali, the mycelium threads stood out as unstained bodies. In order to demonstrate the tubercle bacillus, he therefore treats the section with very weak caustic soda or potash. They are best found in sections of recent tubercle which has been hardened for twenty-

gefunden hat. Ferner aber erscheint auch die diagnostische Bedeutung der Koch-schen Stäbchen von hohem Werth.

"Ich hoffe dass diese beide letzten Sätze, namentlich die Vorletzte durch die Discussion, welche Sie einleiten werden, die allgemeinste Verbreitung in England finden und die Collegen und das Publicum veranlassen werden mit ihrer bewährten Energie alle Consequenzen zu ziehen. Ich empfehle namentlich das Studium und die Bekämpfung der Tuberculose der Rinder als die häufigsten Quelle menschlicher Tuberculose (Scrofula). Mit Gruss der Ihrige

"E. Klebs."

four hours in absolute alcohol. If the tissue is afterwards stained with an alcoholic solution of safranin, the bacilli stand out as unstained bodies. Ziehl describes Baumgarten's method of treating sputum. He says that Baumgarten first treats it with very weak caustic potash, spreads it on a cover glass, dries it, and passes it two or three times through a gas flame. He then stains it with aniline blue, and everything becomes blue except the tubercle bacilli, which remain colourless.

On May 1st 1882, Professor Ehrlich brought forward his method of staining tubercle bacilli, which is now so well known and has been universally adopted. He also holds that an alkaline solution is necessary to stain the bacilli, but instead of potash or soda he uses a saturated watery solution of aniline, to which he adds a saturated alcoholic solution of any of the basic aniline dyes till precipitation commences. The dyes which he specially recommends are fuchsin, methyl violet, and gentian violet, but he states that he has tried all the basic aniline dyes in this way. The sections after being stained in the solution are then decolorised in nitric acid (one part to two of water), and after being washed in water are put in some contrast stain. The contrast stains generally employed are in the case of the red stain in the first instance, methylen blue, and in the case of the violet stain chrysoidin or vesuvin. By this process he obtains quicker staining, more intense staining of the bacilli, less intense staining of the tissue, and hence easier demonstration than by Koch's original method, while he thinks that possibly, also, more bacilli are stained. If other forms of bacteria are present, they are decolorised by the nitric acid and afterwards stained by the contrast dye in contradistinction to the tubercle bacillus.

Ehrlich holds that the relation of these bacilli to the basic aniline dyes is the same as that of other bacteria, but that the difference depends on the presence of a sheath which can be penetrated by colouring matters under the influence of alkalies, while it becomes impenetrable after the use of acids. Hence he draws the conclusion that disinfective substances must be alkaline.

Weigert has made out the best proportions between the saturated alcoholic solution of the aniline dye and the saturated

watery solution of aniline, and thus the difficulty which some have experienced in making Ehrlich's solution has been removed. Weigert's formula is :—

Saturated watery solution of aniline, 100 ccm.

Sat. alcoh. sol. of fuchsin, methyl violet, &c., 11 ccm.

The views of Koch and Ehrlich with regard to the necessity of alkalinity of the solution, and the conclusion of Ehrlich as to the presence of a sheath with peculiar properties were upset by the researches of Dr. Franz Ziehl, published on August 12th, 1882. He found that the tubercle bacilli were stained even though acetic acid were added to the solution in sufficient quantity to make it acid. He further found that aniline left blue and red litmus unaltered, but behaved to a weak reddened solution of phenolphthalein like an acid, the colour disappearing. The staining solution made with an aniline which did not act in this way, did not colour the tubercle bacilli. Hence he concluded that the material which gave this reaction was the cause of the success, but he could not tell what it was, though it probably belonged to the aromatic series. He then tried a substance with like reactions in place of the aniline, viz. carbolic acid. This succeeded, though not quite so quickly as the solution made with aniline. He also succeeded with resorcin and pyrogallic acid in place of aniline. From these facts it is evident that the alkaline reaction of the fluid is immaterial.

Ziehl concludes that it is a property of the tubercle bacillus to take up colouring matters only slowly, to hold them in spite of the action of acids and alkalies, and this taking up of the colouring matter is hastened by certain substances, such as aniline, &c.

I have tried a large number of substances which decolorise a reddened solution of phenolphthalein in place of the aniline, but I find that they act very variously. Ordinary aniline contains toluidine as well as aniline, and I obtained the two substances separate and tested them. I found that the absolutely pure aniline (sold by Hopkin and Williams at 5*s.* an ounce) gave a very brilliant result, better than I had been getting with the ordinary aniline, while toluidine acted very well but hardly so strongly as the ordinary aniline.

I also found that paraniline did very well, but not so well as aniline.

I was able to confirm Ziehl's statements as to carbolic acid which acts almost as satisfactorily as aniline, the difference being hardly perceptible. I found, however, that resorcin and pyrogallic acid did not act well at all.

Nitrobenzole and the alcohols, methylic, propylic, butylic, and amylic, were fairly good, but not nearly so good as aniline.

Phloroglucine acted very well, almost as well as aniline.

Boracic acid acted feebly, but apparently all the bacilli were stained though faintly. This was also the case, though to a less extent, with salicylic acid.

A solution made in Weigert's proportions with water and alcoholic solution of fuchsin, but without aniline, or any substitute for it, washed in nitric acid and stained afterwards in blue, stained a good many bacilli red but not nearly all. It seems to be always the case that a few bacilli hold the fuchsin after washing in nitric acid, even though no aniline or any substitute for it has been employed. Hence I suppose that some of these bacilli differ from others, and that it is probably at a particular stage of growth that they can be stained in this way by fuchsin alone.[1]

All the various substitutes for aniline which I have mentioned decolorise the reddish solution of phenolphthalein with the exception of methylic alcohol, which not only does not decolorise it but intensifies the colour. As methylic alcohol does act to some extent in the same way as aniline and quite as well as the other alcohols of the same group, it is probable that a neutral reaction and the decolorisation of reddened phenolphthalein solution are not essential attributes of a substitute for aniline.

How does the aniline act? It has been supposed that it forms a compound with fuchsin, &c., and that this new stain stains the bacilli. Unfortunately for this theory, when aniline and rosaniline do combine to form a compound the result is blue, not red. And also the fact that such a variety of substances can be

[1] In testing these points I used crushed tubercles or cheesy glands where no micro-organism but the tubercle bacillus was present.

substituted for aniline is opposed to this idea. Further, I have evaporated the Ehrlich-Weigert fuchsin solution both in an incubator and also in the cold and afterwards re-dissolved it, with the result that the solution acted like ordinary fuchsin solution without aniline and not like the original fluid. In evaporating the solution the aniline almost entirely disappeared, showing that it had not formed a compound. (Evaporate 20 grs. of aniline and there is not a residue of $\frac{1}{4}$ grain.)

Nor is it that the presence of aniline is necessary to enable the fuchsin to stain the bacilli, for I have satisfied myself that after staining with fuchsin alone, but before washing with nitric acid, the bacilli are stained.

The fact seems to be that fuchsin or other basic aniline dye alone can stain the bacilli, but that only a few of them, at a certain stage of growth probably, can retain the stain in the presence of strong nitric acid. If aniline or a variety of other substances be, however, added to the staining fluid, the bacilli are then enabled to hold the stain. The aniline seems to act in some way as a fixing agent. I have stained these bacilli first in fuchsin alone and then afterwards immersed them in aniline water, with the result that after washing in nitric acid large numbers of bacilli were seen to be stained, while similar specimens treated with fuchsin alone showed only a few bacilli. The result, however, was not so good as where the anilin and fuchsin acted together.

I have not found any other organism, except the leprosy bacillus, which retains the stain on treatment with nitric acid, but a micrococcus has been met with by Lichtheim which does so. I have, however, seen that if a slide of sputum is immersed in the nitric acid for a very short time only, a torula often retains the colour as well as the tubercle bacillus. A little longer immersion removes the stain from the torula. Psorospermiæ and sometimes the outer coat of some parasites also retain the red stain after washing with nitric acid. Elastic tissue and cheesy matter, the former more especially, retain the red if the sections are only immersed for a short time in the nitric acid.

A great objection to Ehrlich's method is that many sections of delicate tissues, such as thin sections of the eye, shrivel up and

become useless after immersion in the strong nitric acid. I have therefore made a number of experiments with the view of getting rid of the nitric acid, and the following seems to me the best method.

After staining the section in Ehrlich-Weigert's fuchsin solution wash it in distilled water, immerse it in alcohol for a moment and then place it in the following solution:—

Distilled water, 100 ccm.
Saturated alcoholic solution of methylen blue, 20 ccm.
Formic acid (pure), 10 min.

The sections are left in this solution from one to two hours and are then treated in the ordinary manner with water, alcohol, and oil of cloves.

Glacial acetic acid may be substituted for the formic acid, but it does not act quite so well, and a larger quantity (about 12 minims) must be used.

I do not recommend this method for sputum, because it takes longer than the nitric acid method and does not, I think, possess any advantage over it in the case of dry preparations.

The view that tuberculosis may be caused by the introduction of non-tuberculous substances beneath the skin of rabbits and guinea-pigs has been advanced by a number of observers and is widely held, more especially in this country. As this view is opposed to the recent statements that the cause of tuberculosis is a specific micro-organism, it is impossible to omit mention of this important question.

Among the first to oppose the specific view was Dr. Burdon Sanderson. He says at the end of his first report, in 1867, on the communicability of tubercle by inoculation: "From these facts it is evident that these lesions can no longer be regarded as dependent on any property or action possessed by the inoculated material in virtue of its having been taken from the diseased organs of a phthisical patient. This conclusion is rendered almost certain by the result of the following experiment" (that with the seton to be mentioned immediately).

The facts on which Dr. Burdon Sanderson bases his view are that, besides a number of experiments with tuberculous substances, when he inoculated animals with pus from pyæmic abscesses if the animals survived they became tuberculous, and that he inserted a seton of unbleached cotton into each of two guinea-pigs, one of which afterwards became tuberculous. (He also inoculated two animals with material from a diseased supra-renal capsule in a case of Addison's disease, but this was probably tuberculous, or at least strumous, for there were traces of old tubercle in the lung. This experiment would not therefore count.)

In his second report Dr. Burdon Sanderson still holds "that in rodent animals the tuberculous process may originate not merely by the inoculation of tubercle, but by any irritation of the requisite intensity applied to the subcutaneous tissue, and that any external injury, provided that the animal survives its immediate effects, is capable of becoming the first link in a chain of pathological changes which cannot be distinguished from those produced by insertion of tuberculous material." In support of his view he does not mention any fresh experiments, but quotes Cohnheim's experiments, in which tuberculosis followed the introduction of cork, gutta-percha, and other inert materials into the abdomen of animals. At the end, however, an experiment is mentioned which is different from the former one with the seton, and which seems to support the idea of accidental contamination of the material formerly employed. He says, "As regards the question of a specific contagium of tubercle, we think it is very important to note that this is not as yet disproved by the facts of traumatic tuberculosis. It still remains open to inquiry whether or not injuries which are of such a nature that air is completely excluded from contact with the injured part are capable of originating a tuberculous process." And in support of this it is said, "Setons steeped in carbolic acid were inserted in ten guinea-pigs on September 24th, 1868, each animal receiving two. At the same time extensive fractures of both scapulæ were produced on five others, care being taken not to injure the integuments. No tuberculosis or other disease of internal organs resulted in either case."

About the same time Dr. Wilson Fox published a large number of experiments on the same subject with similar results, and in which the number of cases where the inoculation of non-tuberculous substances was followed by tuberculosis was much greater. I may tabulate the certainly non-tuberculous substances, the inoculation of which was followed by the development of tuberculosis:—

	No. of Experiments.	Successful Cases.
Putrid muscle	5	4
Seton	4	1
Cotton thread	3	1
Vaccine fluid	4	4
Pus and lymph from rabbit	8	1
Cirrhotic kidney	1	1
Chronic pneumonia	2	2
Foul pus	5	3
Suppuration injury to knee	1	1
Sloughs	3	1
Pyæmic abscess of spleen	2	2
Same spleen, unaffected part... ...	2	1
Silver wire	24	3
	64	25

That it was true tubercle that was produced in some cases is shown by the successful reinoculation of the tubercles following inoculation of low pneumonia (two experiments), of pneumonia (rabbit, one experiment), and of putrid muscle (three experiments).

Dr. Wilson Fox states distinctly that none of the animals not operated on became tuberculous. The bronchial glands only escaped eight times out of sixty-four. (Dr. Koch says that enlargement of the bronchial glands implies spontaneous infection.)

The inoculations were done by means of a trocar fitted with a piston, but no mention is made of disinfection of the trocar after each injection.

These views seemed at first to receive great support from the experiments of Cohnheim and Fraenckel,[1] who found in the pathological institution at Berlin that all guinea-pigs into whose

[1] Virchow's *Archiv.* vol. xlv.

abdominal cavities they introduced pieces of cork, paper, cotton threads, &c., became tuberculous. Cohnheim has, however, since completely retracted the conclusions derived from these experiments, because he found that he could not produce tuberculosis in the same way in the pathological laboratories of Kiel and Breslau, and because Fraenckel failed also to reproduce the results when the experiments were done at his own house in Berlin. Cohnheim is now one of the most strenuous supporters of the specific nature of tubercle.

Lebert and Waldenburg also opposed the specific nature of the disease, the latter putting forward the view that it was due to special physical properties of the material introduced, and that tubercle is due to particles taken up by the blood and deposited in the tissue. For the criticism of these works I would refer to Klebs' paper.[1]

In contrast to Tappeiner's results from inhalation of tuberculous matter, a paper was published by Schottelius,[2] who stated that miliary nodules in the lung could be produced not only by inhalation of tuberculous sputum, but also by inhalation of organic material which could undergo fermentation in the bronchi and alveoli, such as non-tuberculous sputum, cheese, &c. It is important to note that in no case did he get general tuberculosis, and that he always speaks of the effects as inflammatory. His results do not in reality contradict Tappeiner, for the latter got in some cases tubercles, not only in the lungs, but in other organs.

Numerous researches have been published which contradict the supposed traumatic origin of tuberculosis, indeed most of the recent writers on tuberculosis mention control experiments with non-tuberculous materials which have yielded entirely negative results. I may give as an example of this sort of work the results obtained by Dr. Carl Salomonsen.[3]

Salomonsen introduced the substances to be tested into the anterior chamber of the eyes of rabbits, and watched whether or not tuberculosis of the iris developed. By this method there could be no confusion from the occurrence of spontaneous

[1] Virchow's *Archiv.* vol. xliv., 1866.

[2] *Ibid.* vol. lxxiii.

[3] *Aftryk fra Nord. Med. Arkiv. Bd.* 11, 1879.

tuberculosis, while different substances could be introduced into each eye and the result watched.

His results are divided into four groups, but only the second interest us where supposed non-tuberculous substances were introduced into the eye.

	No. of Experiments.	Length of time during which the animal was observed.
Periosteum of rabbit	1	6 Weeks.
Skin of rabbits	1	6 ,,
Carcinoma of axillary glands	2	2 Months.
Inflammatory induration round ulcer of prepuce	4	2 ,,
Mycotic tumours from tongue of cow	2	3 ,,
Carcinoma of mamma (fatty degeneration) ...	2	4 ,,
Gumma of liver	2	4 ,,
Caseous gland from scrofulous child	2	6 Weeks.
Chronic panophthalmitis from rabbit (lasted 6 months)	5	2 Months.
Muscle of tuberculous guinea-pig	1	4 Weeks.
Testicle ,, ,, ,, ...	1	4 ,,
Kidney ,, ,, ,, ...	1	4 Months.
Caseous nodule from lung of a tuberculous guinea-pig, dried at 100° C., for $1\frac{1}{2}$ hours.	3	3–4 ,,
Caseous nodule from lung of tuberculous guinea-pig, boiled for 6 minutes	3	$4\frac{1}{2}$,,
Caseous nodule from lung of tuberculous guinea-pig, in absolute alcohol for $1\frac{3}{4}$ hours	2	4–5 ,,

In not a single case did tuberculous iritis or general tuberculosis develop.

In contrast to these experiments he publishes a large number with tuberculous material, all of which were followed by tuberculous iritis and, if kept longer, by general tuberculosis at least of the lungs.

As Baumgarten has also investigated this matter, I may enumerate the materials which he has inoculated without in any case obtaining tuberculosis :—All sorts of organic and inorganic foreign bodies, also chemical materials of various kinds, a great variety of pathological new formations, such as carcinoma, sarcoma, myeloid sarcoma, malignant lymphoma, leucæmic tumours, hard and soft chancres, lupus, typhus, glands, actinomycosis, &c., the products of acute and chronic inflammations, healthy and unhealthy pus, croupous and diphtheritic

masses, granulation tissue and scars of different kinds from different situations, cheesy material from various sources, cheesy pus from man and animals, cheesy infarcts, caseous tumours, putrid material in different stages of decomposition, lower organisms of various kinds, gregarinæ, infusoria, cocci and bacteria from various septic fluids, and all kinds of fungi. He states that in some cases injection of bacteric fluid into the eye led to the formation of small tubercle-like masses in internal organs, but these turned out to be small abscesses. In none of the other cases was a single tubercle produced.

As this matter was so important, and as the non-specific view of tuberculosis is still held in England, I have thought it necessary to perform some similar experiments with special precautions. I have taken the greatest care to prevent contamination of the materials used, of the instruments, and of my own hands with tuberculous material. The animals have also been kept under the best possible hygienic conditions.

Experiment I., August 28th, 1882.—Experiments with setons of various kinds :—

1. A seton of cotton thread was put into the back of a black rabbit. This animal remained apparently unaffected. The seton came out at the end of September. The animal was killed November 11th, 1882. Lived seventy-eight days. On post-mortem examination it was found to be well nourished and no disease of any of the organs could be detected. On microscopical examination the lungs, liver, spleen, and kidneys were found to be quite healthy.

2. A little bit of the same cotton thread was put into the anterior chamber of the eye of a red and white rabbit. This animal died September 12th, 1882. Lived fifteen days. All the organs were healthy with the exception of the liver, which was seen to be full of the large cheesy masses so common in rabbits. On microscopical examination, November 1st, 1882, all the organs were healthy except the liver. The liver was full of cheesy masses and psorospermiæ. The appearance of the masses was a cheesy indefinite mass surrounded by a definite wall of dense fibrous tissue. These were recognised as due to psorospermeæ by the fact that when sections were stained with the fuchsin aniline solution in the manner described for tubercle

bacilli, the psorospermeæ remained red while everything else became blue. (See Fig. 6, Plate I.) There was, therefore, no difficulty in recognising them. I consider this fact with regard to the staining of these bodies very important as a means of diagnosis.

3. A seton of thick worsted thread put into the back of a brown rabbit. My last note with regard to this animal was on October 7th (after forty days), when the seton was still in, but there was no inflammation or sore about it, no enlarged glands, and the animal was apparently in perfect health. This rabbit was stolen on October 8th.

4 and 5. Setons of the cotton thread used in Nos. 1 and 2 were put into the backs of two guinea-pigs. These setons had come out at the end of September, and the skin-wound was perfectly healed and there were no enlarged glands. The same condition is noted on October 7th. These animals were stolen during October.

6. A seton of worsted thread was put into the back of a white guinea-pig. No sore or enlarged glands followed, and when the animal was killed on November 2nd, 1882 (after sixty-six days, the seton being still in), all the organs were perfectly healthy. On microscopical examination this result was confirmed.

In this experiment all the animals except No. 2 were under observation quite long enough to enable me to determine whether they would or would not become tuberculous as the result of the insertion of the seton. The result was entirely negative, indeed I expected it, for I have in other experiments often sewn up wounds in rabbits with cotton thread (I should think about fifty times), and left it in for weeks without in any case obtaining tuberculosis.

Experiment II., August 28th, 1882.—Experiment with vaccine lymph taken from the calf by Dr. Renner on August 26th, 1882.

1 and 2. Small pieces of the thick lymph were put into the anterior chamber of the right eyes of two rabbits. On September 9th, 1882, small opacities were found at the upper part of the corneæ of both rabbits, but the iris was perfectly healthy and the animals were well. On October 7th (forty days after inoculation), the opacities had almost disappeared, there was no iris

tuberculosis, and the animals were quite well. They were both stolen on October 8th.

3. A seton of cotton thread was soaked in the lymph and put into the back of a black rabbit. This seton came out at the end of September, and the animal was then perfectly well. It was killed on November 14th, 1882. Lived seventy-eight days. There was no sore, no enlarged glands, the animal was fat, and the organs perfectly healthy. On microscopical examination, the healthy state of the organs was confirmed.

4 and 5. Setons of worsted thread were soaked in the vaccine lymph and put into the backs of two guinea-pigs. These animals remained perfectly well, no sore formed at the point where the seton was, and the neighbouring glands did not enlarge. One guinea-pig disappeared during October, and the other was killed on November 2nd, 1882, and the organs were perfectly healthy. This was confirmed on microscopical observation of the lungs, liver, spleen, and kidneys.

Experiment III., August 28th, 1882.—Experiment with human vaccine lymph obtained from Dr. Sumner on August 28th, 1882.

1 and 2. Lymph was introduced into the anterior chamber of the left eyes of two rabbits. On September 22nd, the upper part of the corneæ in both animals was opaque, but there was no appearance of iris tuberculosis, and the animals were apparently well. This was also the condition on October 7th, but the opacities were evidently rapidly disappearing. Both animals were stolen on October 8th. Observed for forty days.

3. A seton of worsted thread was soaked in the lymph and inserted into the back of a rabbit. This animal was also quite well on October 7th, the seton was still present, but was causing no irritation, and the neighbouring glands were not enlarged. Animal stolen on October 8th.

4. Seton of worsted thread soaked in the lymph and introduced into the back of a guinea-pig. This seton did not seem to cause any irritation, and the glands in the neighbourhood did not enlarge. Animal killed on November 2nd, 1882 (lived sixty-six days), and organs found healthy. On microscopical examination of the lungs, liver, spleen, and kidneys, the healthy state of the organs was confirmed.

5. Seton of worsted thread soaked in the lymph was inserted

into the back of a guinea-pig. This seton came out at the end of September, leaving a sore which soon healed. The neighbouring glands did not enlarge. Animal killed November 12th, 1882. Lived seventy-six days. Nothing was found either macroscopically or microscopically in any of the organs.

The result of these two experiments on ten animals is also perfectly definite and negative. It will be remembered that the four cases in which Dr. Wilson Fox inoculated vaccine-lymph were all followed by tuberculosis. Here vaccine-lymph was inoculated in ten cases without the production of tuberculosis in a single instance, although all the animals were under observation for a sufficient length of time.

Experiment IV., August 28th, 1882.—Experiment on the introduction of various materials into the abdominal cavity of animals.

1. A piece of cork introduced into the abdominal cavity of a brown rabbit. On September 21st this animal seemed ill, and was found dead on the morning of September 22nd. Lived twenty-five days. There were no definite post-mortem appearances; certainly no tubercles in any of the organs. On microscopical examination, the lungs were in parts engorged with blood and the capillaries ruptured. There were also plugs in the vessels which stained very strongly with gentian violet, but I was unable to resolve them into micro-organisms. In the liver were one or two old cicatrices, but no recent changes, no further appearances to account for death. (The animal had diarrhœa for a day or two before death.)

2. A piece of tubercle that had been in alcohol since June 1st, 1882 (see Experiment XII.), was introduced into the abdomen and left eye of a black rabbit. This animal seemed well in the end of September, but was found dead on the morning of October 2nd. Lived thirty-four days. Heart full of cheesy masses and much enlarged. In the lower lobes of the lungs were a number of nodules, the liver contained one or two small cheesy masses, the spleen was much enlarged, but did not contain any nodules, and the kidneys were apparently healthy. Eye healthy. On microscopical examination, the masses in the heart were seen to consist solely of inflammatory tissue which had in part undergone caseous degeneration: the

masses of granulation tissue could be followed among the muscular bundles in a manner radiating from the centre of the inflammation. No epithelioid cells, and no appearance in the least like tubercle. On staining with gentian violet one could see numerous excessively minute granules, which I have no doubt were micrococci from their appearance and arrangement. There were inflammatory masses (indeed regular abscesses) in the lower part of the lungs, with the same micrococci. The cheesy spots in the liver were evidently remnants of old inflammatory processes. There were no tubercle bacilli.

3. A piece of cork introduced into the abdominal cavity of a guinea-pig. No effect followed. The animal was observed till the end of October, and remained perfectly well. It was killed by a dog on November 1st, 1882.

4. Piece of worsted inserted into the abdominal cavity of a guinea-pig. This animal remained apparently quite well till October 7th. It was stolen on October 8th.

5 and 6. Pieces of hardened tubercle (see No. 2) introduced into the abdominal cavities of two guinea-pigs. One of these was killed on November 2nd, 1882 (lived sixty-six days), but the various organs were found healthy; no trace of the original piece of tubercle. On microscopical examination the lungs, liver, spleen, and kidneys were seen to be perfectly healthy. The other guinea-pig died on December 31st, 1882. Lived 125 days. It had diarrhœa. No disease of any organs could be found. On microscopical examination all the organs were healthy, and there was no appearance of tubercle.

The result of this experiment can only be definitely known from four of the animals. Nos. 3 and 4, though apparently well, might have been ill, and as the material was introduced into the abdominal cavity there were no enlarged glands, sores, or iris tuberculosis to guide us. In the four known cases, however, there was no development of tubercle.

Experiment V., November 7th, 1882.—Experiment with pus from the wound of a patient suffering from pyæmia. The pus was thick and foul smelling.

1. One minim was injected into the left eye of a rabbit. Panophthalmos resulted, and the animal was ill for some time. It, however, gradually recovered, and in December was

apparently well. It died on January 10th, 1883. Lived sixty-four days. On post-mortem examination no disease was found anywhere but in the large intestines and cæcum. From the ileocæcal valve, involving the cæcum and extending for about nine inches along the large intestine, were numerous circular whitish patches, about the size of a pea, with greyish points in the centre and projecting somewhat above the level of the peritoneal coat. On laying open the intestine there was over each of these patches a circular ulcer, or rather a hole in the mucous membrane. On microscopical examination, part of the mass was seen to be cheesy and the rest inflammatory. Interspersed throughout were large numbers of psorosperimæ ; some of these remained red on staining for tubercle bacilli, others were colourless. There were no tubercle bacilli or other bacteria.

2. Five minims of the pus were injected subcutaneously into the back of a guinea-pig. January 23rd, 1883 : This animal was found dead to-day. It had diarrhœa. Lived seventy-seven days. On post-mortem examination all the organs were seen to be healthy, and there were no tubercles.

3. Four minims of the pus were injected into the abdominal cavity of a guinea-pig. This animal did not seem to suffer anything as the result of the injection, and when examined on January 28th, 1883 (after eighty-two days), it was quite well and strong.

I may state before proceeding to the description of the results obtained in the experiments with micro-organisms that all the organs of the animals that have died or been killed have been carefully examined microscopically for the presence of tubercle bacilli, for the presence of other bacteria, and for the histological appearances. For tubercle bacilli I have used Ehrlich's fuchsin solution made according to Weigert's formula, staining afterwards with methylen blue ; for other micro-organisms I have used in addition gentian violet ; and for histological structure I have as a rule employed, as well as the foregoing stains, Ehrlich's triple staining, with hæmatoxylin, rubin S., and orange. In speaking of the number of bacilli present in a specimen I have drawn up a scale, reference to which will save

much repetition. I say they are in *enormous masses* when there are great masses containing innumerable bacilli; *very numerous* when they are too numerous to count; *numerous,* about forty or fifty in a field; *considerable numbers,* about twenty or thirty in a field; in *moderate numbers,* about ten in a field; *few,* two or three in a field; *very few,* only one in two or three fields. These numbers refer to extremely thin sections made with Williams's microtome. Of course, if a section is thick, a comparatively small number of bacilli in each plane would seem many on focusing.

The tubercle bacilli vary considerably in length, the longest being about the $\frac{1}{7000}$th of an inch. They are narrow (about $\frac{1}{5}$th or $\frac{1}{6}$th of their length), more or less rounded at the ends, and they generally present a sort of beaded appearance, clear spots with intermediate stained parts, the rod outside the clear spots being also stained (Plate II., Fig. 10). The number of beads in a single rod varies from four to eight, and is on an average six. The rods are generally straight, but they are not uncommonly more or less curved. In tissue they are generally found singly, or sometimes in pairs, united at their ends or stuck together side by side. At other times there are two or three lying across each other, the axis of all being more or less in the same direction.

In cultivations they are as a rule shorter, and stuck together in dense masses. Perhaps their shortness is due to their being broken when spread out on the glass, but I think they are really shorter when growing rapidly than when growing slowly. (*Vide* examination of Koch's case of injection into the veins.) According to Koch they are motionless.

I may also add that in all these experiments I have used Koch's syringes and observed all the precautions described by him in their purification, &c. The animals constituting each experiment have been kept in the same hutch, which was separated from the others by an interval, and there was no possibility of communication between the animals.

As I have already stated, Prof. Toussaint gave me portions of the organs of various animals illustrating the results of his experiments, and also tubes containing the cultivations themselves. I will now describe the results of the observations which I have made with these materials.

(*a*) Examination of the organs received:—

1. *July 22nd*, 1882.—A rabbit which was paralysed in its hind legs, and which he was sure would have died to-morrow was killed to-day. The following is the history of the experiment. In December, 1881, the juice squeezed out from a muscle of a cow affected with tuberculosis was kept for ten minutes at a temperature of 55° C., and then injected into a pig. This pig died 147 days later with extensive tuberculosis, and a bit of the tuberculous material was inoculated subcutaneously into two rabbits on April 7th. One of these rabbits died two days ago, and this one would have died to-morrow.

At the seat of inoculation there was an ulcer, beneath which were numerous tubercles, the neighbouring glands were enlarged and caseous, and there was very extensive tuberculosis of various organs (lungs, kidneys, spleen, mediastinum, diaphragm, costal pleura), but very little macroscopically in the liver. Portions of several of the organs were at once put into a bottle containing alcohol.

On October 3rd, 1882, I made sections of these organs with the following result:—

Lung.—Almost entirely converted into a tuberculous mass, presenting the same appearances as other cases of artificial tuberculosis to be afterwards described, a good deal of caseation, tubercle bacilli present in moderate and considerable numbers, having all the characters of those described by Koch. No other micro-organisms.

Kidney.—Large caseating tuberculous masses in the cortical part. Tubercle bacilli in considerable numbers in some parts and numerous in others. No other micro-organisms.

Spleen.—A considerable number of caseating tuberculous masses. In some of these the tubercle bacilli are very numerous. No other micro-organisms.

Lymphatic gland.—Almost entirely caseous, and containing numerous tubercle bacilli. No other micro-organisms.

2. Portion of kidney from a rabbit inoculated on December 30th, 1881, and died on March 31st, 1882. In this case some of the tenth cultivation of micrococci from the blood of a rabbit which had been inoculated with tuberculous material from a

cow, was injected at the base of the right ear. The kidney and other organs were extensively tuberculous.

January 20th, 1883.—The kidney was badly preserved, and stained badly, so that the histological structure could hardly be made out. There were large tuberculous masses in the cortex and the tubercle bacilli, which were as a rule numerous in these masses, stained very well. No other micro-organisms.

3. Piece of lung from a rabbit inoculated on November 29th, 1881, with the fifteenth and sixteenth cultivations from a tuberculous rabbit, and later on, on February 11th, 1882, with the twenty-first and twenty-second cultivations of the same series. This animal died on April 19th, 1882, and extensive tuberculosis of the various organs was found.

January 20th, 1883.—The lung was extensively infiltrated with tubercles, presenting the usual appearances to be afterwards described. Tubercle bacilli were found in some places in enormous numbers. Epithelioid cells in alveoli and bacilli in the cells (see after). In one or two places there were very interesting appearances from the point of view of the spread of the disease. In one large blood-vessel there was a mass of cells lining the wall and projecting in one part into the calibre of the vessel though not obliterating it entirely. In this mass there were numbers of tubercle bacilli, there were also some in the wall of the vessel (Plate I., Fig. 1). In two other places there were numbers of bacilli in the wall of vessels (apparently arteries) penetrating quite to the inner coat. No other organisms.

4. Organs of a cat with the following history. A pig ate part of the organs of a cow affected with tuberculosis, and became tuberculous. A drop of the blood of this pig was put into a vessel containing rabbit infusion, and micrococci developed. Ten drops of the seventh cultivation of these micrococci were injected into the peritoneal cavity of this cat, which died 109 days later with enlarged abdominal glands, and tubercles in liver, spleen, lungs. The tubercles were not large, because, according to Toussaint, the enlarged glands caused the death of the animal by arresting the flow of lymph before there was time for the tubercles to grow to a large size.

October 3rd, 1882.—Examination of organs received:—

Lung.—Typical tuberculosis generally in considerable masses,

with here and there slight caseation, a good many giant cells. Tubercle bacilli present in moderate numbers, and several seen also in the giant cells. No other micro-organisms.

Liver.—Several tubercles in the lobules, but not very numerous, commencing caseation. Tubercle bacilli present in moderate numbers. No other micro-organisms.

Spleen.—Only a few tubercles. Tubercle bacilli present, but very few. The spleen did not stain very well. No other micro-organisms.

Lymphatic gland.—For the most part caseous. The tubercle bacilli are in many places very numerous. No other micro-organisms.

5. Lung of a pig with following history. A portion of the tuberculous material from the cat just mentioned was inoculated into a rabbit, and caused tuberculosis. From the tubercles in the rabbit this pig was inoculated, and it died sixty-seven days afterwards.

October 3rd, 1882.—The tubercles in the lung are still for the most part very small, but their structure does not seem to differ in any way from that of tubercle in the lung of rabbits. Tubercle bacilli are present in moderate numbers. In a number of very thin sections of small tubercles I found an average of nine bacilli in each. No other micro-organisms.

(*b*) I also received from Toussaint two tubes containing the micrococci with which he had been working, and with these the following experiments were done. Before describing these I may state that when I opened each tube I dried a small quantity of the fluid on cover-glasses, and stained it both by the ordinary methods for staining bacteria, and also by the methods for demonstrating tubercle bacilli. I did not find tubercle bacilli in either of the tubes, and they seemed to contain nothing but micrococci, very minute, but staining well with gentian violet, vesuvin, and methylen blue, like other forms of micrococci. They were chiefly in large groups, but there were also a few pairs, threes and fours, grouped in the manner characteristic of micrococci (see Fig. 5, Plate I.). There were a few bodies more ovalish, and somewhat larger, which might be small bacteria.

No. I.—A tube of serum containing micrococci. This was the fourteenth cultivation from the blood of a tuberculous

rabbit, and the flask was inoculated in January, 1882. Toussaint said that the micrococci would be still alive and active, and he cited in proof thereof the fact that the cultivation from which the cat, whose organs I have referred to above, was inoculated, was six months old. The fluid was clear, and there was a thick, white deposit at the bottom which, when shaken up, made the fluid muddy, and was composed of the micrococci.

August 29th, 1882.—Further cultivation from this tube. The tube was opened, as great care as possible being taken to prevent contamination, and six tubes containing solidified sheep's serum, and three flasks containing meat infusion, were inoculated and placed in an incubator kept at 37° C.

Development occurred in all the serum tubes very rapidly. They stood in the incubator till September 21st, when I removed them, and allowed them to stand at the ordinary temperature. They were carefully examined on September 27th, with the following result. They were numbered 1 to 6. In tubes 1 to 5 inclusive the surface of the serum was covered by a thin growth consisting of numerous little gelatinous masses, which had more or less run together, but did not penetrate the surface of the serum nor affect its solidity.

No. 1.—Contained micrococci and a few ovalish bodies.

No. 2.—Micrococci apparently quite pure.

No. 3.—Micrococci, and also very short oval bacteria, and a few longer rods.

No. 4.—Micrococci alone.

No. 5.—Micrococci alone.

No. 6.—Long delicate bacilli with spores. The serum had become fluid on the surface. This was probably accidental contamination during the re-inoculation.

Nothing developed in the flasks of meat infusion.

Experiment VI., August 29th, 1882.—Injection of the contents of this tube into animals. About a drop was injected into the anterior chamber of the left eyes of each of three rabbits, and about three drops into the abdominal cavity of each of two guinea-pigs.

September 22nd, 1882.—The eyes of the three rabbits are all right. No iris tuberculosis, and only small opacities of the cornea where the needle was inserted. Both the guinea-pigs are well.

October 8th, 1882.—The opacities in the eyes of the rabbits were smaller and hardly perceptible. The eyes were carefully examined and were seen to be perfectly normal. The guinea-pigs were well and fat.

On October 9th the three rabbits and one guinea-pig were stolen.

November 2nd, 1882.—The remaining guinea-pig was killed to-day (lived sixty-five days). It was quite fat, and macroscopically there was no evidence of disease in any of the organs.

A later microscopical examination of the lungs, liver, spleen, and kidneys, showed them to be perfectly healthy. No trace of tubercle, either recent or old.

The result of this experiment is quite definite. In none of the animals was tuberculosis produced, though all were under observation for a sufficient length of time (the three rabbits and one guinea-pig for forty days, and one guinea-pig for sixty-five days).

Experiment VII., September 30th, 1882.—Inoculation of the organisms grown on the solidified blood serum.

The growth covering the surface of the serum was in the case of each tube scraped off by means of platinum wires purified by heat every time they were used. This material was then mixed up thoroughly with a small quantity of boiled distilled water, and injected into the animals by means of a purified syringe—a fresh syringe being used for each injection.

No. 1.—A rabbit was inoculated from tube 1 into the right eye, and nine minims were injected into its back. On October 8th the cornea was seen to be somewhat opaque, but the animal seemed well.

October 15th, 1882.—Animal died to-day (lived fifteen days). Cornea opaque, no tuberculosis of the iris. No trace of the point of injection in the back. A few cheesy masses in liver. Other organs healthy.

October 27th, 1882.—Organs examined. No tubercles or tubercular bacilli in any organ. A few old cheesy masses in the liver. No bacteria of any kind.

No. 2.—A rabbit was inoculated from tube 2 into the left eye, and eight minims were injected into the back. This animal

died on October 2nd (lived three days). No special post-mortem appearances, and nothing morbid was found on microscopical examination of the organs.

No. 3.—A rabbit was inoculated from tube 3 into the right eye, and six minims were injected into its back. On October 8th the animal seemed well, and there was only a little opacity of the cornea at the point of inoculation. It was stolen on October 9th.

No. 4.—A rabbit was inoculated from tube 4 into the left eye, and five minims were injected into its back. It died on October 2nd (lived three days). No special post-mortem appearances and nothing was found on microscopical examination.

No. 5.—A rabbit was inoculated from tube 5 into the right eye, and six minims were injected into its back. Died October 8th (lived nine days). Nothing found on post-mortem or microscopical examination.

No. 6.—A rabbit was inoculated from tube 6 into the left eye, and seven minims were injected into its back. Died on October 2nd (lived three days). Nothing found on post-mortem or microscopical examination.

No. 7.—Ten minims of a mixture of the organisms from all six flasks were injected into the abdominal cavity of a guinea-pig. This animal was examined at various dates and seemed always to be in good health. It was found dead on the morning of December 6th (lived sixty-seven days). On post-mortem examination the organs were found to be perfectly healthy, and no trace of tubercles, and this was confirmed on careful microscopical examination.

The result of this experiment can only be told from No. 7, in which a mixture of the growth from all the tubes failed to produce tuberculosis. The rabbits were a very young brood—too young for purposes of experiment. I did not ascertain this, however, till everything was prepared, and it was too late to delay the experiment. I do not think that the organisms injected had anything to do with the death of the animals.

No. II.—A tube of serum containing micrococci. This was the twenty-third cultivation from the blood of a pig, and this flask was inoculated in February, 1882.

Experiment VIII., October 29th, 1882.—The contents of the tube were well shaken up, and the whole as far as possible (about four minims) was injected into the abdominal cavity of a young cat. This animal has been under observation ever since, and is, and has been, perfectly well.

At the same time about a drop of the same fluid was injected into the back of a mouse. This mouse died on November 16th (lived eighteen days), but no tubercles were found on post-mortem or microscopical examination.

The injection of the cat was done at the special request of Prof. Toussaint, who said that he had found the result of the injection of these micrococci more successful in cats than in rabbits. The result was, however, negative.

October 29th, 1882.—Cultivation of these micrococci.

Four solidified serum tubes and two tubes containing gelatinised meat infusion were inoculated from this tube. The serum tubes were put in the incubator. They were numbered 1 to 4.

Examined on November 16th, 1882.

No. 1.—Contains micrococci and long spore-bearing bacilli.

No. 2.—Contains micrococci.

No. 3.—Contains short bacilli.

No. 4.—Contains micrococci.

In one gelatine tube micrococci alone developed; in the other micrococci and long bacilli.

Experiment IX., November 7th, 1882.—Inoculation of the growth in these tubes.

(*a*) Growth scraped off the surface of the serum in tube 1, and rubbed up with boiled distilled water. About two drops injected into the left eye of a rabbit, and eight minims into the abdomen of a guinea-pig.

(*b*) A piece of the growth from tube 2 put into the left eye of a rabbit by means of a purified wire passed through an incision in the cornea.

(*c*) Growth from all four tubes mixed together, rubbed up with boiled distilled water, and two drops injected into the right eye of a rabbit, and eight minims into the abdomen of guinea-pig.

November 12th, 1882.—All the animals well. In all the rabbits there was a little lymph in the pupil, and partial opacity of the cornea.

November 23*rd*, 1882.—All the animals well. No inflammation about the eyes of the rabbits, and the opacities of the cornea have almost gone. Traumatic cataract in *b*. No tubercular iritis.

December 10*th*, 1882.—All well.

December 28*th*, 1882.—All well; but one guinea-pig was missing this morning. It was apparently quite well (under observation fifty-one days).

January 28*th*, 1883.—The three rabbits and one guinea-pig are apparently quite well. There are now no opacities in the corneæ of the rabbits.

February 8*th*, 1883.—One rabbit killed. All the organs perfectly healthy. Other animals well.

The result of this experiment is that five animals inoculated with the organisms cultivated from the second of Toussaint's tubes remained well without the development of tubercle.

Dr. Koch gave me portions of the organs of several animals for further examination, and also cultivations of bacilli for experimental purposes. While in Berlin I made sections of a considerable amount of the material in his laboratory; the result of this examination will be referred to later.

(*a*) Examination of the organs obtained from Dr. Koch. I may state that I will refer in detail at the end to the histology of tubercle as elucidated by these researches, and shall therefore not describe the points in giving the results of the examination of each animal.

1. Organs of rabbit, No. 2 p. 250, inoculated into eye with cultivation from a spontaneous monkey tuberculosis. Lived twenty-six days.

Eye.—Iris converted into a diffuse tuberculous mass, and containing a considerable number of tubercles. There were numerous tubercles in the lungs, liver, and spleen, those in the liver being still quite young and exhibiting very well the structure of young tubercle to be afterwards described. In the lung the tubercle bacilli were numerous and very numerous. In the liver they were also numerous. In the spleen they were few.

2. Lung of rabbit, No. 4, p. 250. Inoculated in the same way as the former. Lived thirty days.

Numerous tubercles throughout the lung. Not large. No caseation. Only a few bacilli. (Plate II. Fig. 13). No other micro-organisms.

3. Guinea-pig which had a cultivation of bacilli injected into its thigh six months previously; was killed on August 1st. It looked fat and well, but had tubercles in the lungs and spleen, not in other organs. It was apparently recovering. Tubercles from the lungs were crushed and examined at the time, but no bacilli were seen.

January 4th, 1883.—*Spleen* examined. Contains a few tubercles but no caseation. Only very few bacilli.

4. About Christmas 1881, a syringeful of a cultivation of bacilli was injected into the abdominal cavities of two dogs. One of these became very ill and was killed four weeks later, when tuberculosis of various organs, more especially the abdominal organs, was found. The other dog was ill for a time but got better. At the beginning of June, 1882, two syringefuls of cultivation of bacilli (seventh cultivation from a man, the last cultivation having been done six months previously) was injected into its abdominal cavity. The animal became ill, got steadily thinner, and when killed (August 7th, 1882) it was very emaciated and hardly able to move about. Killed August 7th. On post-mortem examination there was found ascites, well marked tuberculosis of the abdominal wall somewhat resembling perlsucht, matting together of the mesentery, thickening of the omentum with tubercles in it, tuberculosis of liver, kidneys, peritoneum, and lungs, but less advanced in the lungs than in the liver. There was also an ulcer on the skin at the seat of injection affecting the skin only and not communicating with the abdominal cavity. The morbid process was centred in the abdominal cavity. A nodule from the liver, squeezed between two cover-glasses, showed tubercle bacilli. Animal lived eight weeks.

January 4th, 1883.—*Liver* examined. The tissue stains badly, but the bacilli stand out beautifully. The capsule is slightly thickened with little nodules in parts. Everywhere throughout the capsule the bacilli are numerous, and are often growing in

little clusters. The liver is full of tubercle, the masses being as a rule continuous with each other and penetrating in all directions among the liver cells, but definitely marked off from them. There are enormous numbers of bacilli which are generally in twos and threes, side by side, or in larger masses or in circlets. They seem to have spread in from the capsule along the tracts of connective tissue. In one part the tuberculous tract was traced right across the liver with dilations at intervals. The bile-ducts were inclosed in the centre of the masses. The bacilli were mainly in epithelioid cells, and a few were seen in liver cells at a little distance from the masses. Some of these might however have been in cells in the capillaries.

Lung.—Contained a large number of tubercles and numerous bacilli. The bacilli were contained almost entirely in epithelioid cells sometimes 6–10 in one cell. These epithelioid cells seemed to be derived from the alveolar epithelium, and in one bronchus where the epithelium was in the main intact, I found numerous bacilli in the mucus lining it. No cavities in the lungs, and no caseation.

5. The rabbit which received the injection into the vein of the ear on August 2nd, killed August 10th. Lived eight days. (See *a* p. 249.)

January 23rd, 1883.—*Lungs* examined. A large number of small tubercles in the lungs. Bacilli as a rule numerous in the nodules. They are shorter than usual and are often united together in dense masses. They present the appearances seen on examination of cultivations rather than those seen in the tissues. In parts, however, where they are growing in the epithelioid cells, and not in the blood-vessels, they present the normal appearance.

The spleen and kidneys were apparently perfectly normal. In the liver, however, I saw one or two minute plugs in the capillaries of the liver consisting of five or six white blood corpuscles with a few bacilli. I also saw in one section one or two bacilli at the margin of and probably inside the liver cells.

6. Perlsucht nodule from a cow. Well developed fibrous tissue and numerous giant cells, also a considerable tract of caseous material. The bacilli were very numerous in all the

giant cells and also in the caseous material where radiating groups of them could often be seen, looking as if they had previously been in the interior of a giant cell which had disappeared leaving the bacilli. A few bacilli were also scattered through the fibrous tissue.

7. Tuberculous nodule from a horse. The histological appearances and the number of bacilli were identical with No. 6, except that giant cells were more numerous.

(*b*) I also received from Dr. Koch several tubes containing bacilli growing on blood-serum. These have been carefully examined microscopically, and nothing but tubercle bacilli have been found; no other micro-organisms.

No. I.—A tube of solidified serum containing tubercle bacilli. This was the ninth cultivation from the lung of a man who died of phthisis on January 23rd, 1882, and this tube was inoculated on July 25th, 1882.

Experiment X., August 28th, 1882.—The growth was scraped off the surface of the serum and rubbed up in a pure mortar with boiled distilled water. The following injections were then made, a purified syringe being employed for each experiment:—

(*a*) One or two drops injected into the anterior chamber of the left eye of a black rabbit.

September 23rd, 1882.—This rabbit has well-marked tuberculosis of the iris and is thinner.

October 8th, 1882.—The cornea has now become opaque, and the iris can no longer be seen.

November 12th, 1882.—Large cheesy mass projecting from the eye, more especially at the seat of inoculation. Animal rather thin, but not apparently ill.

November 26th, 1882.—Animal killed to-day. Lived ninety days. Left eye converted into caseous mass. (For detailed description of the eye in this and all the following cases, see Mr. Jennings Milles' report, which will appear later). Slight enlargement of the glands in the neck, chiefly on left side. No caseation. Lung full of tubercular masses, more especially at the bases where these masses are confluent. All the masses have commencing caseation in the centre. No cavities in the lungs. Bronchial glands not enlarged. No other organs apparently affected.

December 31*st*, 1882.—Examined microscopically. *Lungs* full of tubercular masses, which, however, contain few bacilli. No other micro-organisms.

Liver, spleen, and kidneys healthy.

(*b*) One or two drops injected into left eye of a brown and white rabbit.

September 23*rd*, 1882.—Left eye completely converted into a caseous mass. Animal thin. Killed September 30th, 1882. Lived thirty-three days.

On microscopical examination the *liver*, *spleen*, and *kidneys* were found to be healthy. In the *lungs* I found in several sections only two small tubercles. These contained very few bacilli which were only in epithelioid cells apparently in alveoli. The tubercles were in immediate contact with masses of lymphatic tissue, one being close to a bronchus, and the other close to a large vessel.

Mr. Milles found the eye converted into a tuberculous mass, containing few tubercle bacilli, and also another form of bacilli staining blue.

(*c*) The point of the needle of the syringe was introduced into the left eye of a brown rabbit, but the piston was not moved.

September 9*th*, 1882, died. Lived twelve days.

There was no appearance of tubercle of the iris, and on examination of the various organs microscopically they were found healthy with the exception of the liver, which was full of gregarinæ.

(*d*) Nine drops of the mixture of cultivation and boiled distilled water were injected into the abdominal cavity of a white guinea-pig.

This animal died September 15th, 1882. Lived eighteen days. Lumbar lymphatic glands enlarged, and some of them caseous. Tubercles in liver, spleen, and lung; none in kidney.

Liver examined October 24, 1882. There were tubercles in the substance of the liver, of all sizes and in all stages of growth. The bacilli were few or in moderate numbers, on an average four to six, in a thin section of a tubercle. The liver cells could be distinctly traced into the interior of the tubercles in the lobules of the liver, and the bacilli were generally in these cells. I also found them in several places, apparently in liver

cells where no tubercle yet existed, but, as a rule, the nucleus of these cells had subdivided, and a commencing tubercle was apparent. I also saw several bacilli, apparently in intercellular spaces, close to the surface of the capsule.

Lung.—In the lung the tubercles were not so numerous nor so far advanced. The bacilli were few in number.

Spleen.—In the spleen there were a good many tubercles and a considerable number of bacilli.

Glands.—More or less completely converted into tuberculous masses, with caseation here and there. Numerous bacilli.

Experiment XI., October 30th, 1882.—Portion of cheesy material from the eye of rabbit *b*, Experiment X., inoculated under the skin of the axillary region of a guinea-pig, and a piece of lung containing what I thought might be a tubercle introduced under the skin of the abdomen.

A tuberculous sore formed in the axillary region, and the axillary glands became enlarged. I had intended to kill the animal about the middle of December, as it was getting thin and evidently tuberculous, but it disappeared on December 8th.

Other two animals were inoculated at the same time, but one (a rabbit of the same brood as Experiment III.) died three days later, and the other, a guinea-pig, was stolen on October 8th.

The result in the one case showed that the tuberculous material in the eye could produce tuberculosis when re-inoculated.

Experiment XII., November 28th, 1882.—Inoculation of animals with tubercles from the lungs of the black rabbit (*a*, Experiment X.).

A rabbit had portions of the tubercles put into the eyes. The result is that the animal has now (January 28th, 1883) tuberculous iritis, and apparently general tuberculosis, but as the experiment has reference to points not alluded to in the present report, I will not give the details of it in the meantime.

A guinea-pig was inoculated subcutaneously in the groin with tubercle from the lungs. There is now (January 28th, 1883) a tuberculous sore and enlarged glands in the groin, and the animal is getting thin.

February 12th, 1883.—This animal died to-day. Tuberculous sore, tuberculous glands, tubercle in lungs, liver, spleen, intestines, and kidneys.

Another guinea-pig and two rabbits were also inoculated at the same time, but these died in two, eight, and fourteen days respectively, before there was time for the development of tubercle. In the animal which lived fourteen days there was apparently commencing tuberculosis of the iris in the neighbourhood of the piece of tissue inoculated, but it has not yet been examined microscopically.

This experiment likewise shows that the tuberculosis induced by the injection of bacilli in Experiment X. was re-inoculable in the same manner as tubercle produced by the inoculation of tuberculous material.

No. II.—A tube of solidified serum containing the same bacilli as No. I. (the ninth cultivation from the lung of a man who died on January 23rd, 1882).

Experiment XIII., November 2nd, 1882—Growth scraped off surface of serum and rubbed up with boiled distilled water as usual. A little of the pure material injected into the right eye of three rabbits. The piston in these syringes did not fit tightly, and almost no fluid was injected. Into the left eye the following material was injected :—

Into No. 1.—One part of the fluid containing bacilli mixed with one part of 1 to 1000 watery solution of bichloride of mercury. The bichloride of mercury solution was allowed to act for eight minutes before the mixture was injected.

No. 2.—One part of the fluid containing bacilli and one part of a one per cent. solution of resorcin in water. The resorcin solution acted for nine minutes before injection.

No. 3.—One part of the fluid containing bacilli and one part of a five per cent. watery solution of carbolic acid. The carbolic acid solution acted for twelve minutes before injection.

Result in No. 1.—This animal was killed on December 15th, 1882. Lived forty-three days. Up till a fortnight previously there was no appearance of tuberculous iritis in either eye, but when killed it was found that the left iris was tuberculous, the right eye being buphthalmic but not tuberculous. In the left eye the tuberculosis had apparently developed from the neighbourhood of the point of inoculation ; the upper part of the iris was completely infiltrated with tuberculous material, and there were nodules round about; the lower part of the iris was almost free.

There were a few tubercles in the lungs, but the other organs were apparently healthy.

January 10*th*, 1883.—On microscopical examination all the organs except the lungs were found to be healthy. In the lungs there were very few tubercles, and these were very young. They only contained a few bacilli, and these were always in epithelioid cells. These cells were apparently derived from the alveolar epithelium, filled up the alveoli, and there was also increase of the inter-alveolar tissue.

Mr. Milles reports tubercles in the left eye. Tubercle bacilli few.

Result in No. 2.—On November 23rd, 1882, there was tubercular iritis in the right eye, but the left was still clear. On December 10th, 1882, the cornea of the right eye had become opaque. Iris could not be seen. Rabbit looked ill. There was tubercular iritis in the left eye.

January 7*th*, 1883.—Animal died to-day. Lived sixty-six days. Both eyes highly tuberculous. Cornea opaque and protruding, and neither iris could be seen. Cheesy glands under the jaw. Lungs excessively tuberculous; tubercle of kidneys, but not large or numerous. Apparently none in the spleen or other organs.

January 21*st*, 1883.—Microscopical examination. *Lung* very tuberculous; numerous spots of caseation. Few bacilli. In one or two places at the margin of cheesy matter there are a good many bacilli, but as a rule they are few. A few commencing tubercles in the *liver* with few bacilli. Tubercles in *spleen*, but few bacilli. Also tubercles in the cortical part of the *kidney*. Here the bacilli were rather more numerous than in the other organs, and some were seen among masses of cells in the kidney tubercles.

Mr. Milles found tubercles in both eyes. Tubercle bacilli few.

Result in No. 3.—Tubercular iritis developed in both eyes, but considerably later in the left than in the right (about twelve days). Examined on January 28th, 1883 (after eighty-seven days); both eyes now converted into tuberculous masses. Animal thin and undoubtedly tuberculous.

February 2*nd*, 1883.—Rabbit died. Extensive tuberculosis of

lung. Tubercles in spleen and kidneys. Also a few in the liver.

Microscopical examination :—*Lungs ;* tuberculous masses with bacilli in parts in masses, and in parts in moderate numbers. *Spleen ;* large and small tubercles. Bacilli numerous and very numerous in the cheesy parts, in the young tubercles in moderate or considerable numbers, and there in the epithelioid cells. *Liver ;* numerous small tubercles containing bacilli in moderate or considerable numbers. *Kidney ;* large and small tubercles in the cortical part containing bacilli in considerable numbers and numerous.

No. 4.—Three or four minims of the fluid containing bacilli were injected into the abdominal cavity of a guinea-pig, and the right eye was also inoculated. Tubercular iritis developed in the eye. On January 28th, 1883, eye completely tuberculous. Tuberculous sore on abdomen at seat of injection, and the neighbouring lymphatic glands greatly enlarged. Animal thin.

February 18*th,* 1882.—Killed to-day. Tubercles in lungs, liver, and spleen.

No. III.—Tube of serum containing tubercle bacilli. This was the fifth cultivation from a case of perlsucht which died March 20th, 1882. The inoculation was made from a nodule on the diaphragm.

Experiment XIV., November 2*nd,* 1882.—The bacilli were rubbed up with boiled distilled water as usual. A little of the pure material was injected into the right eyes of three rabbits. Into the left eyes the following materials were injected :—

No. 1.—One part of this fluid containing bacilli was mixed with one part of a 1 per 1000 watery solution of bichloride of mercury. This mixture was allowed to stand for twelve minutes, and then injected into the left eye of No. 1.

No. 2.—One part of the fluid containing bacilli was mixed with one part of a one per cent. watery solution of resorcin. This stood fourteen minutes, and was then injected into the left eye of No. 2.

No. 3.—One part of the fluid containing bacilli was mixed with one part of a five per cent. watery solution of carbolic acid. This stood fifteen minutes, and was then injected into the left eye of No. 3.

Result in No. 1.—On November 23rd, 1882, it was found that

there was a well-developed tubercular iritis in the right eye, but apparently nothing in the left. On December 10th, 1882, the left eye was beginning to show appearances of tubercular iritis; the right eye had become converted into a caseous mass. This animal died on January 7th, 1883. Lived sixty-six days. Right eye highly tuberculous, and converted into a caseous mass. In left eye tubercular iritis, but cornea still clear. Cheesy glands on each side below the jaw. Very extensive tuberculosis of the lungs. Tubercles in liver, kidneys, and spleen. Tubercles in large intestine and omentum. Enlarged and caseous mesenteric and lumbar glands. Tubercle of bones of the skull, but no tubercle in the brain or meninges.

Microscopical appearances: January 21st, 1883:—

Lung.—Full of tubercular masses leaving very few healthy alveoli. These masses contain enormous numbers of bacilli. There is not much caseation. At the margin of the confluent masses, and throughout the lung generally, the alveoli are filled with epithelioid cells and contain numerous bacilli. (Plate II., Fig. 12.) In one place part of the wall of a bronchus has become tuberculous, and the bacilli are seen growing luxuriantly in the epithelium lining it. In many of the alveoli around, the epithelial cells are swollen and generally contain bacilli, and in many parts of the lung bacilli are seen in alveolar epithelium which is apparently almost healthy.

Kidney.—Contains several large tubercular masses in the cortical part which are full of bacilli. No tubercles in the medullary part. At the margin of the masses the bacilli are seen in the kidney tubules both in the canal and in the epithelium (Plate II., Fig. 9), and in one case I found a small cluster of bacilli in the interior of a tubule at a considerable distance from any cheesy mass.

Liver.—Here the process is not nearly so far advanced as in the other organs. There are only small tubercles in the shape of small nodules containing epithelioid cells and numerous bacilli. On looking over the sections many liver cells are seen to contain bacilli where no tubercle yet exists. Some bacilli are, I think, also in the capillaries, but it is difficult to determine whether they are in capillaries or in the adjacent liver cells, they lie so close to the edge of the cells.

Spleen.—Contained tubercles and numerous bacilli.

Intestine.—There are nodules in the mucous tissue containing numerous bacilli. The tuberculous masses extend around the intestinal glands, but the bacilli do not seem to grow in the glandular epithelium. I only once saw bacilli in the interior of a gland duct.

Mr. Milles found tubercles in both eyes containing tubercle bacilli in enormous numbers.

Result in No. 2.—This animal was killed on November 26th, 1882. Lived twenty-four days. Tubercular iritis in both eyes, but most advanced in the right in which the cornea is becoming opaque. Nothing seen in the organs.

On microscopical examination of the organs they were found to be healthy.

Mr Milles found tubercles in both eyes containing large numbers of bacilli.

Result in No. 3.—This animal was killed on December 15th, 1882. Lived forty-three days. Both eyes very tuberculous, but right most advanced. (Tubercular iritis appeared about a week earlier in the right than in the left eye.) Cornea vascular in both except at the centre, and the pupils cannot be seen. A considerable number of minute tubercles in both lungs, but only one or two with commencing caseation. A few tubercles in the liver and also in the spleen which was enlarged. Other organs healthy.

Microscopical examination on January 15th, 1883, showed a few tubercles in *lungs, liver,* and *spleen.* They were fewest in the *liver,* where only one or two were seen. In the *lung* they were present in considerable numbers, and showed the ordinary appearances of tubercles. The bacilli were found only in the epithelioid cells, and were numerous in all the tubercles. In one alveolus there was a large giant cell filling it up entirely and containing bacilli. (Plate I., Fig. 7.)

About three minims of the fluid containing bacilli were injected under the skin of the abdomen of a guinea-pig. This animal was killed December 21st, 1882. Lived forty-nine days.

A sore was found at the seat of puncture, and a cheesy patch in the neighbourhood. There were large cheesy glands in the

left groin, and smaller glands, not cheesy, in the right groin. There were one or two grey points in the spleen. Nothing apparently in the liver or other organs. Calcareous and cheesy glands below the stomach.

January 27th, 1883.—Examined microscopically; the organs were found to be healthy. The inguinal glands were tuberculous and cheesy, and contained bacilli in moderate numbers.

Two minims were injected into the abdominal cavity of a guinea-pig. Animal still alive on January 28th, 1883. On examination a large tuberculous sore and enlargement of the neighbouring lymphatic glands were found.

February 7th, 1883.—This guinea-pig was killed and there were found a tuberculous sore at the seat of injection, enlarged and cheesy inguinal glands, extensive tuberculosis of spleen and liver, a good many tubercles in the lungs.

I have inoculated a number of animals with tuberculous material chiefly with the view of obtaining tubercles for cultivation purposes; but as these experiments illustrate the constant presence of bacilli in tubercle, I may mention them here.

Experiment XV., April 9th, 1882.—A guinea-pig was inoculated subcutaneously with a piece of diseased synovial membrane from the amputated knee-joint of a patient who had strumous disease of the knee and phthisis.

June 1st, 1882.—This animal was killed and the spleen, lungs, and liver were tuberculous. On microscopical examination these organs were found to contain tubercles and tubercle bacilli in considerable numbers.

Experiment XVI., June 17th, 1882.—Two guinea-pigs were inoculated subcutaneously with portions of a tuberculous lung (Mrs. M.'s) which contained a considerable number of bacilli. One of these died August 12th, 1882, during my absence, and was not examined. The other died August 16th, 1882, and was found to have a tuberculous sore on the back, enlarged axillary glands and tubercles in liver, lungs, and spleen.

September 27th, 1882.—Examined microscopically. A number of large and small tubercles in the *lung* containing a considerable number of bacilli. Several tubercles in the *spleen* with few bacilli, and also young tubercles in the *liver* with few bacilli. In

the liver there were appearances which indicated that those liver cells which contained bacilli might develop into giant cells.

Experiment XVII., August 17th, 1882.—A portion of the spleen of this guinea-pig was inoculated subcutaneously into a rabbit. An abscess had been induced on the side of this rabbit in February, 1882, by means which need not now be detailed, as they belong to a series of unpublished experiments on another subject. This abscess burst, and there remained a cavity containing a cheesy mass. This cheesy mass still remained when the animal was inoculated with tubercle, but the rabbit was apparently in perfect health. This animal died on October 25th, 1882. Lived sixty-nine days. On post-mortem examination there was a small ulcer at the point of inoculation and the neighbouring glands were cheesy. There were numerous small, cheesy nodules in the lungs. Other organs apparently healthy.

December 21st, 1882.—On microscopical examination, small tubercles were found in the lungs, and also largish masses with commencing caseation. Tubercle bacilli few. Other organs healthy.

Experiment XVIII., August 28th, 1882. — (*a*) Phthisical sputum containing a moderate number of tubercle bacilli was injected subcutaneously into a rabbit and a guinea-pig.

The result with regard to the rabbit was, that on September 23rd, 1882, a sore was found at the seat of injection, and a cord extending down towards the inguinal region. Inguinal glands on that side enlarged. On October 7th the same condition was found, and the animal was getting thin. The rabbit was stolen on October 8th.

In the case of the guinea-pig, a sore formed at the seat of inoculation, and the glands in the neighbouring inguinal region became enlarged. The animal was killed on December 2nd, 1882. Lived ninety-six days. The left inguinal glands and the lumbar glands were enlarged. There was extensive tuberculosis of the spleen, liver, mesentery, and lungs. The kidneys were apparently healthy. There was not much emaciation. Almost all the tubercles had cheesy centres.

In the uterus there was found an almost full-grown fœtus. The placenta and the organs of the fœtus were apparently healthy.

On microscopical examination of the adult animal, the following appearances were found:—

Lungs—large and numerous tubercles with cheesy centres.

Spleen—very tuberculous.

Kidney—healthy.

Liver—small tubercles.

Gland (*Inguinal*) — completely tuberculous and in parts cheesy.

There were bacilli in all the tubercles, but they were few in number, except in the gland, where they were numerous. Section of the ulcer of the skin showed cheesy masses underneath containing a considerable number of bacilli.

Careful microscopical examination of the placenta and the organs of the fœtus showed no tubercles and no bacilli.

(*b*) Phthisical sputum from another source, containing rather fewer bacilli, was inoculated subcutaneously into a rabbit and a guinea-pig. The rabbit died three days later, but on microscopical examination no cause was found.

In the case of the guinea-pig, a sore formed at the seat of injection (middle of the back), and the inguinal glands on both sides became enlarged.

October 19*th*, 1882.—Animal died: lived fifty-two days. Besides the sore and the enlarged inguinal and lumbar glands there were tubercles in the spleen and also a few in the lungs.

November 14*th*, 1882.—Microscopical examination showed a few tubercles in the lungs and spleen and also very few in the liver. Bacilli few in the tubercles generally, only one or two in each section of a tubercle. A considerable number of bacilli in the enlarged lumbar glands.

Experiment XIX., *October* 31*st*, 1882.—A guinea-pig was inoculated with tubercular material from a phthisical lung. (Two rabbits inoculated at the same time died before there was time for the development of tuberculosis.)

November 23*rd*, 1882.—The guinea-pig died. Lived twenty-three days. Enlarged cheesy inguinal glands and enlarged lumbar glands. Organs apparently healthy. On microscopical examination the various organs were found healthy, but the glands were tuberculous and contained in parts a considerable number of bacilli.

Experiment XX.—Inoculation of three mice with portions of phthisical lung failed to produce tuberculosis, but the incisions in the skin were too large and the material may have slipped out.

I inoculated a mouse with bacilli grown on serum and produced tuberculosis, but as I have lost the notes I cannot give the particulars.

I have also examined a considerable amount of material both in Berlin and here.

In Berlin Dr. Koch placed the following tissues at my disposal, and the result of my investigation of them is as follows :—

1. Lung of a cow affected with perlsucht. Numerous caseous nodules with enormous numbers of bacilli ; the bacilli are most numerous in the caseous part and in the giant cells. No other micro-organisms.

2. Perlsucht (nodule from pericardium). Masses of cells with cheesy patches, a few giant cells, enormous masses of bacilli both in the tissue and in the giant cells.

3. Perlsucht (lung of a cow). Tubercular infiltration with cheesy degeneration in patches ; bacilli in places in enormous masses, very numerous inside the giant cells (which are few in number), where they are arranged circularly just inside the nuclei.

4. Tubercle of horse (gland). A large number of giant cells, and cheesy and non-cheesy tissue ; bacilli very numerous, especially in giant cells and cheesy matter.

5. Tubercle of horse (lung). Tubercles in the lung containing bacilli in considerable numbers.

6. Tubercle of hen (spontaneous). Intestine : enormous masses of bacilli in the nodule surrounding the intestine.

7. Liver of first dog inoculated with bacilli at Christmas, 1881, and killed four weeks later (see No. 4, p. 278). Liver full of small tubercles without caseation ; bacilli present in moderate and considerable numbers.

8. Rabbit inoculated in the eye with cultivation of bacilli from monkey tuberculosis. *Lung* full of tuberculous nodules in which the bacilli are numerous ; *liver* contains numerous young tubercles with numerous bacilli ; *spleen* tuberculous, bacilli numerous and very numerous.

9. Rabbit inoculated with old cultivation of bacilli from man. *Lung* full of tubercular masses ; bacilli present in parts in

enormous numbers and masses, especially in alveoli. *Liver;* tubercles in and between the lobules, with bacilli in considerable numbers and numerous.

10. Phthisis cavernosus. Bacilli present in considerable numbers in the walls of cavities and in the alveoli, which were full of cheesy material.

11. Phthisis. Great development of fibrous tissue, almost no caseation; bacilli very few.

12. Acute miliary tuberculosis. Bronchial gland. This gland was cheesy and contained enormous numbers of bacilli both in the cheesy material and at the margin. In some places in the cheesy part there were circles of bacilli as if they had lain radially in giant cells.

13. Acute miliary tuberculosis. Bronchial gland. This was in the main caseous, and in three places there were enormous numbers of bacilli and there were also numerous bacilli throughout the cheesy part. Dr. Koch drew my attention to an artery surrounded by enormous masses of bacilli which infiltrated its walls and extended quite to its interior. The lumen of the artery was still patent, and thus the bacilli could enter the blood in great numbers. Koch looked on this as the source of the acute affection. There were also a number of plugs of micrococci in the vessels, but they had not spread into the tissue and did not seem to be causing inflammation. Koch said that these entered by an ulcer in the tongue, and he thought that they probably hastened the fatal result.

14. Acute miliary tuberculosis. Lung. Tubercles containing very few bacilli.

15 to 19. A number of specimens given to Dr. Koch by Prof. Weigert. No history, but probably from cases of acute tuberculosis.

Tubercle of thyroid. Tubercular masses in thyroid with a few giant cells and caseation; bacilli present in considerable numbers.

Tubercle of bladder. Tubercular ulceration of bladder, the surface being cheesy; bacilli very numerous in the cheesy matter, but they diminish rapidly in numbers as one passes from the free surface.

Tubercle of suprarenal capsule. A number of tubercles with

caseation in parts confluent; bacilli present in considerable numbers.

Tubercle of kidney. Tubercles in the cortical part of the kidney, bacilli in moderate and considerable numbers.

Tubercle of pons. Tubercle at the surface of the pons in the immediate vicinity of the pia mater; bacilli in moderate and considerable numbers.

20. Tuberculosis of the tongue (Ehrlich's case). Numerous tubercles and tubercular infiltration, chiefly sub-mucous; in some there is commencing caseation and a good many large giant cells; the bacilli are numerous in many places, especially where there is commencing caseation.

21. Tuberculous testicle (extirpated). Tubercles between the septa containing very few bacilli.

22. Cervical gland (extirpated). Scrofula; very extensive caseation; at the margin of the caseous part there are bacilli, but very few.

23. Srofulous glands. Extensive caseation; bacilli very few.

24. Scrofulous axillary gland. Gland full of caseous masses; bacilli extremely few.

25. Synovial disease of finger amputated by Dr. Koch in 1878. Contains a considerable number of giant cells; bacilli very few.

I have also obtained the following materials in London and examined them with the view of ascertaining the presence of tubercle bacilli or other micro-organisms and their relation to the morbid process:—

26. Mrs. M. A rapid case of phthisis. Large caseous masses in the lung, and the alveoli in the neighbourhood full of cheesy material; bacilli in considerable numbers in the alveoli and also at the margin of the cheesy masses.

27. D. Also a rapid case of phthisis. Small cavities lined with caseous matter containing a considerable number of bacilli and surrounded by inflammatory material.

28. W. Acute phthisis. Large cavity at right apex; small cavity at left apex; both lungs infiltrated with grey and partly cheesy tubercles; on microscopical examination these tubercles contained very few bacilli.

29. Lung from a chronic case of phthisis. Very extensive

tracts of fibrous tissue, with one or two caseous spots; very few bacilli, and only in the cheesy parts.

30. A. P. Phthisis of about eight months' duration. Large cavities throughout left lung; small cavities and tubercular masses in upper part of right lung. Chiefly the so-called fibrous phthisis with here and there cheesy masses. In one place in the centre of a cheesy patch there were enormous masses of bacilli (Plate I., Fig. 2), and around, in the cheesy matter, the bacilli were numerous. Near this mass, also in the midst of the cheesy patch, there was a piece of inflammatory tissue, apparently the remains of the wall of an alveolus, quite isolated from the walls of the patch. In one or two other cheesy parts the bacilli were in considerable numbers in the centre. There were no bacilli except in the cheesy matter. Throughout the fibrous tissue were numerous giant cells, but in none of them could I see bacilli. These cells were apparently developing into blood-vessels. In some the processes almost joined each other (Plate II., Fig. 8), and in one I saw a distinct central space which apparently contained red blood-corpuscles.

31. S. D. Phthisis. History unknown. Large fibrous masses with caseation, and at the margin alveoli filled with cheesy matter; bacilli very few; some giant cells only containing pigment granules, but no bacilli.

32. Case of very rapid phthisis (thirteen weeks). Masses chiefly caseous; bacilli very few, chiefly in the cheesy matter and two or three in giant cells.

33 to 35. Three cases of potter's phthisis, varying in rapidity from several months to three or four years. The rapid case showed a large amount of cheesy degeneration, and in the caseous masses and immediately adjacent, epithelioid tissue. The bacilli were present in considerable numbers (Plate II., Fig. 11). In one alveolus filled with large epithelioid cells I saw several bacilli in almost every cell. No bacilli in the fibroid parts.

In the most chronic case there was great development of fibrous tissue and only a few cheesy masses and alveoli filled with cheesy material. In the latter the bacilli were present in moderate numbers. There were a number of giant cells in the fibroid tissue, but these only contained pigment. No bacilli.

In the intermediate case there was more cheesy matter and

the bacilli were present in considerable numbers in it, but absent in the fibrous tissue and in the giant cells. In one part in the cheesy matter at the margin of a small cavity I found enormous masses of bacilli (Plate I., Fig. 3).

36. Miner's phthisis. I did not get the history. Large masses of pigment in the trabeculæ and great thickening of the trabeculæ; fibrous tissue well developed and very vascular; in some alveoli the epithelium is increased in amount and the cells large, and in parts there are patches of embryonic tissue with plenty of vessels; no caseation; not at all tubercular in appearance; no tubercle bacilli.

For the opportunity of examining the last four cases I am indebted to Dr. Heron, to whom they were sent by Dr. G. S. Hatton, of the North Staffordshire Infirmary.

37. Lung from a case of acute tuberculosis. Commencing caseation in the nodules; bacilli few; in some of the neighbouring vessels there were plugs of micrococci, but these had no relation to the tubercles.

38. Case of acute phthisis. Patient had suffered from lupus for years, and the lupus was still extending about the nose. Strumous ulcer in neck; enlarged glands; small cavities at apices of lung; lung infiltrated with small tubercles; tubercular ulceration of intestine; enlargement of mesenteric glands in the vicinity; other organs healthy.

Apex of Lung.—Fibrous tissue with caseation; bacilli as a rule very few, but in several of the cheesy parts and adjoining epithelioid cells they were present in moderate numbers.

Intestine.—Masses of epithelioid cells surrounded by leucocytes in the sub-mucous tissue; in the epithelioid part bacilli were present in considerable numbers; in some of the masses caseation had occurred, and there were a few bacilli among the cheesy matter.

Mesenteric Gland.—For the most part cheesy and containing a moderate number of bacilli around the margin of the caseous part.

Nothing in liver, spleen, or kidney. I could not find any bacilli in the ulcer of the skin.

39. Acute phthisis. Patient admitted moribund, and no history obtained. Alveoli full of cheesy matter containing bacilli in moderate numbers and few.

40. Acute phthisis. Cavities in both lungs with grey granulations around, but getting few towards base; bacilli few except at the margins of cavities, where they were in places in considerable numbers.

41. Disease of the synovial membrane of the knee-joint (the case from which the animal in Exp. XV. was inoculated). A considerable number of giant cells, some containing a bacillus; bacilli few.

42. Case of old phthisis supposed to have been cured. Round fibrous masses, apparently old tubercles; no caseation; no tubercular tissue; no bacilli.

43. Synovial degeneration of the ankle-joint (amputation). Bacilli present, but extremely few, and only in giant cells.

44. Case where there was apparently a primary tuberculosis of the small and large intestine. Enlarged and caseous mesenteric glands and slight affection of apices of both lungs. Lungs examined: tuberculosis with few bacilli. Three glands examined: all cheesy, and contain bacilli in considerable numbers and numerous.

(Since this report was handed in I have examined the lung and liver from a case of acute tuberculosis, and found tubercles with the same histological appearances as in artificial tuberculosis; bacilli few.—Also the spleen, lung, and intestinal ulcer from another case of acute tuberculosis; bacilli few except in intestine, where they were in considerable numbers.—The cheesy material from three strumous joints (two knees and one hip) previously unopened, containing few bacilli.—The diseased synovial membrane from one of these joints; bacilli present, but very few.—And the lungs, spleen, and cheesy gland from a case of monkey tuberculosis; bacilli few and in moderate numbers.)

In the present report I have not touched on my own cultivations. They are not sufficiently advanced to admit of a report, but I may state that I have cultivations of tubercle bacilli now growing, and that I have failed to cultivate micrococci as described by Klebs, Toussaint, and Schüller.

The experiments which I have performed on the induction of tuberculosis by inoculation of non-tuberculous substances, and

which were done under the best hygienic conditions, with care as to complete isolation of the animals, and with special attention to sources of contamination of instruments, &c., with tuberculous material, gave entirely negative results. I may summarise them here. In six cases setons of various kinds were introduced; in ten vaccine lymph, both from the calf and from man, was employed; in three pyæmic pus was injected into the eye subcutaneously, and into the abdominal cavity; and in six various materials (cork, hardened tubercle, and worsted thread) were introduced into the abdominal cavity. Not one of the twenty-five animals (the result in twenty-three being known) became tuberculous. In addition to these experiments I have cited a further fact against setons, viz. that I have often (in about fifty cases) stitched up wounds in rabbits and guinea-pigs with cotton-thread, which was left in sometimes for weeks, and that not in a single case did tuberculosis follow. And I may also add all the cases in which experiments were done with Toussaint's material, micro-organisms being present, (thirteen animals having been under observation for a sufficient length of time), without the production of tubercle in any case. Then I have, from time to time, found animals with cheesy patches in their livers, probably due to psorospermiæ, and though they contained this cheesy material in their bodies they did not become tuberculous. I have also in several cases, not yet published, excited abscesses by injection of lanthanum, of croton oil, and other irritants, these abscesses exhibiting the tendency to caseation so common in abscesses in rabbits, and in no instance has tuberculosis followed. In connection with this I may instance the rabbit mentioned in Experiment XVII., where a cheesy abscess had existed for months, and where it was not till the inoculation of the specific tuberculous material that tuberculosis occurred.

What then is the explanation of the contradictory results obtained by former observers? In the first place I may point out that where microscopical examination of all the organs has not been made, cheesy masses, not tubercular, might easily be taken for tubercles; and even where a microscopical examination was made the accuracy of the diagnosis would depend greatly on the methods of staining employed, and the views which the observer

held as to what constituted a tubercle. I may call attention to some observations which I have made in this research :—

(1) The cheesy masses in rabbits' livers, due to psorospermiæ,[1] might easily be mistaken for tubercles, especially while the fibrous envelope around them is being formed, while it is more or less in the condition of granulation tissue. There, one would find a central cheesy mass surrounded by indifferent cells, which, on some of the views as to the structure of a tubercle, might be regarded as tuberculous. In one or two cases which I have seen I could easily understand this mistake, but I had no difficulty in coming to a conclusion, for the psorospermiæ stood out as red bodies among the cheesy material when the sections were stained by the fuchsin and methylen blue in the manner required for demonstrating the tubercle bacilli (Plate I. Fig. 6). Further, in the lung of rabbit No. 1, Experiment XIII., there was, in addition to the young tubercles already mentioned, a body which was apparently a parasite. The body was the centre of a round mass which, till I examined it more closely, I thought was a tubercle. The centre of the body did not stain and was apparently caseous. Around it was seen a narrow, red coat with peculiar transverse striation, not complete all round, and here and there in the body were red spines resembling the hooklets of an echinococcus, though larger. At one end the body formed a sort of head, the outline of which was marked here and there by pieces of red-stained membrane (see Plate I. Fig. 4). Whatever it may be, had the method of staining not been that employed for tubercle bacilli it would have appeared as a cheesy mass in the centre of granulation tissue, and might have been mistaken for a tubercle.

(2) In the case of rabbit No. 2, Experiment IV., there were inflammatory masses in the heart and lungs which, on macroscopical examination, presented all the appearance of tubercles.

(3) I may also call attention to the changes produced in the lungs by the ova of the strongylus vasorum described by Laulanié, whose preparations I have seen. Here the strongylus is the centre of a nodule containing epithelioid cells surrounded

[1] According to Eimer the psorospermiæ found in rabbits' livers are encapsuled gregarinæ.

by inflammatory tissue, but the nature of the nodule is easily made out by the presence of the strongylus in a blood-vessel in the centre.

(4) There are generally present in the lungs of guinea-pigs small masses of lymphatic tissue in the immediate vicinity of large vessels or bronchi, and if the section is made at a place where these are present in considerable numbers (at the root of the lung) and of considerable size the observer who regards tubercle as a lymphatic growth may see tubercles in lungs where none exist.

While it is well to bear these facts in mind as possible explanations of many cases of supposed tuberculosis as the result of injury or inoculation of non-tuberculous material, it is not, I believe, the most frequent explanation. In Dr. Wilson Fox's paper he describes sores at the point of inoculation and enlarged glands in the vicinity implying true tuberculosis, while this was demonstrated in several instances by the fact that inoculation of the tubercles thus produced again caused tuberculosis. Now it has been shown that inoculation of the inflammatory masses produced in the lungs by inhalation or intravenous injection is not followed by tuberculosis. With regard to the early experiments on this subject it must, however, be remembered that at that time the communicability of tubercle by mediate contagion was not recognised, and as the precautions necessary for thorough disinfection of instruments, &c., had not yet been made out, the channels for the possible introduction of specific micro-organisms were left unguarded.

As to the particular form of organism associated with tuberculosis, three observers, Klebs, Schüller, and Toussaint, describe micrococci which they have been able to cultivate, and with the cultivations of which they state that they have produced the disease. Only one of these, Schüller, has seen the organisms in the tubercles thus produced, and in tubercular tissue generally. As I have only been able to see Toussaint's experiments and results, and as he has gone much further than the other observers, it will be sufficient if I discuss his work alone in detail.

Toussaint's method is, as has already been described, to cultivate organisms (micrococci) from the blood of tuberculous animals, and after repeated cultivations to inject the organisms

into other animals. I received from Toussaint tubes containing these micrococci (Plate I. Fig. 5), which he said were still living, as, indeed, they proved to be. I have inoculated animals from them, and from the cultivations which I have obtained from them. The material obtained from Toussaint was injected into three rabbits, two guinea-pigs, one cat, and one mouse, and of these seven animals six were under observation for a sufficient length of time for the development, at least, of local tuberculosis, and in not a single instance did tuberculosis follow. I have also cultivated the micrococci obtained from him, and injected them into nine rabbits and three guinea-pigs; of these, four rabbits and three guinea-pigs were under observation for a sufficient length of time without the development of tuberculosis in any of the animals. The total result is that thirteen animals were inoculated with the micrococci with which Toussaint works, obtained from Toussaint himself, and in no case did tuberculosis follow. Had these thirteen animals been inoculated with bovine tuberculosis, which is the original source of Toussaint's tuberculosis, there is no doubt that they would all have become tuberculous.

I also obtained from Professor Toussaint a number of tuberculous organs from animals on which he had experimented; six of these were from three animals which had been inoculated with micrococcal fluid of the seventh,—tenth,—fifteenth, sixteenth, twenty-first, and twenty-second generations respectively, and five were from two animals inoculated with tuberculous material. I thus had the opportunity of examining the result in five animals from Toussaint's series of experiments. In all of these tubercles were present, differing in no respect from those always produced by inoculation of tuberculous material. I have spent much time in searching for micrococci by all known methods, but I have found none. I had specimens of Toussaint's micrococci by me, and thus knew the size of the organisms, which do not differ in their relation to staining agents or in their grouping from other forms of micrococci with which I have worked for a long time. I can therefore say definitely that there were no micrococci in the organs given me by Toussaint, whether taken from animals which had been inoculated with tuberculous material, or from animals which had become tuberculous after the injection

of micrococcal fluid. On the other hand, the tubercle bacilli were present in all the organs, in some in large numbers, corresponding to the result found, as a rule, after inoculation of bovine tuberculosis (Plate I. Fig. 1).

I have also attempted to grow micrococci from the blood of tuberculous animals by Toussaint's method, taking every precaution against accidental contamination, and I have entirely failed to do so.

From these facts I conclude that the micrococci described by Toussaint do not cause tuberculosis, and therefore his results must be due to some other agency. The constant presence in the organs of his animals of the bacilli described by Koch is, in my opinion, sufficient explanation of the occurrence of tuberculosis. How are Toussaint's results to be explained? He cultivates micrococci from the blood of tuberculous animals, and after injection of these micrococci he sometimes obtains tuberculosis. On this point I may throw out the following suggestions. The growth of micrococci in the materials inoculated may depend either on fault in experimentation, or, as I believe to be more likely from the constancy of the results, on the presence of large numbers of these organisms in the air of the room in which the experiments are done (*vide* Mr. Lister's and Professor Tyndall's experiences). The success of some of the inoculations may depend on two causes, either on growth of tubercle bacilli, as well as micrococci, in the fluids which yield successful results, or on contamination of the micrococcal fluid during injection. It may be that bacilli grow, though very slowly and imperfectly, in the fluids, and perhaps better in serum than in rabbit infusion; hence the greater success with the former. The growth of bacilli need not necessarily be very luxuriant, for Toussaint injects so much fluid that if they grow at all he would probably get some in ten drops of the fluid. On the other hand, however, Toussaint trusts greatly to carbolic acid as a disinfecting agent for the purification of the instruments employed in inoculations. Now, carbolic acid, though apparently effectual for the destruction of the ordinary forms of micro-organisms, as evidenced by the satisfactory results obtained from its use in aseptic surgery, has been shown to be ineffectual against the spores of bacilli, unless it acts for a long time. The bacillus of

tubercle apparently produces spores, and there is no reason to suppose that these are less resistant than those of bacillus anthracis and other bacilli. Indeed, as will be seen from Experiments XIII. and XIV., a saturated watery solution of carbolic acid, even though it acts as long as fifteen minutes, is not sufficient to arrest the development of the tubercle bacilli, and therefore the washing of a syringe with carbolic acid is not such a certain means of disinfection as was formerly supposed. I think that imperfect disinfection of the syringes employed for injection of the micrococci and of tuberculous blood, &c., is a possible explanation of the results which ought not to be left out of account.

In the researches of Klebs and Schüller a pure cultivation was not obtained, nor were the cultivations carried beyond the third generation, and Schüller was not always successful in producing tuberculosis by the injection of his cultivations. Schüller has seen micrococci in tubercles artificially produced, and he showed me drawings of them, but no one else has been able to find them, although some of the best microscopists and those who have done most with micro-organisms (Koch, Weigert, &c.) have searched for them most assiduously. I have taken great care in examining the various sections which I have made, but I have not found micrococci in the tubercles in any case. In only two instances have I found micrococci at all. These were both cases of acute tuberculosis, and there the micrococci were not present in the tubercles, but in vessels which had no relation to the tubercles. They were present, either having got in through some ulcer, as in Koch's case, and though quite independent of the disease, probably hastened the death of the patient, or they may have been present simply as the result of lowered vitality on the part of the patient, as I pointed out some years ago might occur, though in that case I have never found them as plugs in the vessels.

In Koch's research, the results are much more definite than any previously obtained. He also cultivates micro-organisms from tubercle, but now it is no longer the fact that he only sometimes succeeds in causing tuberculosis and that the tuberculosis thus produced occurs as slowly, or more slowly, than inoculation of tuberculous material. The result of the inoculation

of his cultivations is certain and the disease is more rapid in its commencement than after inoculation of tuberculous matter.

I have given so fully the details of my visit to him, and the result of the investigations which I have carried on with the materials obtained, that I need only refer very shortly to the facts. In Berlin I was able to see a large number of cultivations from a great variety of sources, all presenting the same appearances and containing tubercle bacilli alone. I examined some of these in Berlin and have examined those I brought back with me, and I find nothing but tubercle bacilli, no other micro-organisms, no remnants of the original tissue or caseous material. Indeed, the method of cultivation is such as soon to get rid of all the original cheesy material—a tubercle is crushed and at numerous points on the surface of the serum little masses are seen to appear. One of these masses is picked up and crushed out thoroughly over a large surface of fresh serum. Again fresh masses appear all over this serum and one of these is again taken and crushed out and so on. Thus the original cheesy material is very soon lost and nothing remains but the organisms which have developed from it. The serum in the tubes may be allowed to dry up, as occurred in one of my tubes, but still the bacilli grow when transferred to fresh serum and produce tuberculosis when inoculated into an animal.

The result on animals of inoculation with the bacilli cultivated in this way is certain, and tuberculosis always follows. I may refer to the cases which Dr. Koch inoculated in my presence (four animals) and to the animals which I have inoculated here (twelve animals having lived long enough) all of which became tuberculous and that more rapidly than after inoculation of tuberculous material. The tubercles produced in these cases were infective, and caused tuberculosis in other animals.

On the examination of tuberculous material these bacilli are always found, though in varying numbers. They are always most numerous in bovine tuberculosis, and least numerous in human tuberculosis. I have examined a large number of organs of tuberculous animals and of cases of human tuberculosis, and in all of these, without exception, I have found tubercle bacilli, though the material has been got from many different sources. (I do not include among the latter cases the

miner's lung and the case of supposed cured phthisis, neither of which were tuberculous). I have also examined a number of morbid growths for these bacilli, and have not found them.

The fact that the inoculation of the cultivated bacilli is so certain and rapid in its effects can only be explained on the supposition that in these cultivations we have the virus of the disease in a more or less pure state, and in large amount. But as the only thing which we see multiplying on the serum are these bacilli, and as they are also constantly present in tubercles, it is difficult to see what other conclusion can be come to than that they constitute the virus. This view is still further supported by former experiments on the inoculation of tuberculous material, for it has been found that the result of inoculation is most uncertain when strumous glands and lupus are employed, while it is most certain in the case of phthisical sputum and bovine tuberculosis. Now, in strumous glands there is plenty of cheesy material, plenty of tuberculous material (and the same is the case in phthisis), but there are extremely few bacilli, and the result is slow and uncertain. On the other hand, in bovine tuberculous material there are large numbers of bacilli, and the result is rapid and certain, while in sputum the bacilli are free, and here also the result is rapid. The certainty of the production of tuberculosis by inoculation of tuberculous material, and the rapidity of its occurrence, are in a direct ratio to the number of bacilli present in the original material, and have no relation to the amount of caseous or other tuberculous matter introduced. And the most rapid and efficacious of anything which has yet been tried is the inoculation of the tubercle bacilli growing free on serum. Introduce the point of a syringe which contains fluid in which these bacilli are present into the anterior chamber of the eye without moving the piston of the syringe, and the result will, in all probability, be tuberculosis of the iris, which, however, will be slow in appearing, for but few bacilli were introduced. Inject a small quantity of the same fluid and the result is a rapid appearance of tubercles on the iris (see the case inoculated before me by Koch). Introduce the point of a syringe containing cheesy or other tuberculous material, but only few bacilli, and the chances are that an extremely small proportion of the animals would be infected, for it has been found that the

introduction of a considerable amount of such material is extremely doubtful in its result, and when tuberculosis does occur it resembles the case where the point of the syringe was introduced and the piston not moved rather than the case where a small quantity of the fluid containing bacilli was injected. Again (see Experiments XIII. and XIV.), the effect of germicides, which have not acted long enough to penetrate dense masses or to destroy spores, is to delay the appearance of tuberculosis, because some at least of the bacilli (and probably the most active ones) have been destroyed. The conclusion is, I think, that we have before us in these bacilli, the virus of the acute tuberculosis caused in the lower animals by the inoculation of tuberculous material.

Professor Klebs in his letter makes two objections to Koch's facts, and apparently still upholds his views that a micrococcus and not a bacillus is the real cause of tuberculosis. The first of his objections is that these bacilli may be crystals. This objection is not a formidable one; for any one acquainted with micro-organisms can tell on examination that this is an organism and not a crystal. (Plate II. Fig. 10.) Its appearance, the apparent presence of spores, its variable lengths, its frequent curved form, its arrangement, its behaviour with staining agents, and above all its growth on serum, demonstrate its living nature. It is difficult to conceive that a few crystals put on serum will grow and extend from one point with the rapidity and in the mode that these bacilli spread, and I know of no better test, in spite of Klebs's assertion to the contrary, for a living organism than its increase and multiplication when placed on a suitable soil. They are not any kind of fat crystals, for they are not found in cheesy matter which is not tubercular; and they vary greatly in number in tubercular material itself, sometimes only one being present in two or three fields of the microscope, sometimes a large number. The presence of motion is not a constant attribute of bacteria; indeed, if that were necessary to prove the bacteric nature of a rod, many bacilli, such as the bacillus anthracis, would be looked on as crystalline. Nor is the observation of division under the eye of the observer necessary for arriving at a conclusion as to the presence of life, for there are but few

organisms which have been seen to divide, and nevertheless, there are a great many microscopic bodies which we look upon as living. The best test that we possess is growth under suitable circumstances, and this proof Koch has supplied with regard to the tubercle bacillus. Klebs, however, though he starts this view, himself looks on these rods as bacilli.

That Koch does grow the cause of tuberculosis in the lower animals all who have seen his work or done similar work are prepared to admit, even Klebs and Schüller, who have described other organisms, but in his letter Klebs makes a second objection. He says that he has seen, even in Koch's cultivations, finely granular matter which appears to possess the characters of micrococci. He does not say how he observed this, whether it was in looking at the organisms as they lay on the serum, or whether he spread out the bacilli on a glass and stained them and these round bodies as well. We cannot, therefore, be quite certain what has been seen. If one looks at the organisms as they are growing on a slide covered with solidified serum, one of course sees granular matter; but this is the granular matter of the coagulated serum. On the other hand, I have examined the masses of bacilli when squeezed out between cover-glasses dried and stained, and I can say that I have failed, even though I have taken the greatest care in the preparation and in the search, to find any finely granular matter in any way resembling micrococci. Further, micrococci growing on serum present quite a different appearance to the cultivations of these bacilli. They grow as whitish soft masses, and much more rapidly. One cannot attach much importance to granular matter of an indefinite nature. Bad lenses, bad illumination, and many other causes will show granular matter where none exists. Unless the granules have a definite form and arrangement, or stain well, one cannot speak of them as micrococci.

Aufrecht, in 1881 (*Pathologische Mittheilungen*), describes the centre of the tubercle as composed not of cheesy matter, but "of very small micrococci, of micrococci united in twos and threes to form short chains, and of very short rods." He now considers these short rods to be the same as Koch's bacilli. This however is not the case, for he says that in sputum they

may be stained by a $\frac{1}{2}$ per 1000 watery solution of fuchsin; Koch's tubercle bacilli cannot be stained by this solution.

Leaving these questions, we must now consider what is the gain as regards human pathology from all the researches which have been carried on on tuberculosis, and especially from the demonstration of the tubercle bacillus. All that has as yet been absolutely *proved* is that a variety of materials in man which we class together as tuberculous, produce, when inoculated into rabbits, guinea-pigs, and other animals, acute tuberculosis, and that this also occurs from the inoculation of bovine tuberculosis. Koch's researches further demonstrate that this result is due only to the tubercle bacilli which were present in the materials inoculated. It therefore remains for inquiry in what relation these bacilli stand to the morbid processes in man and in cattle in which they are found.

In man we have the disease termed acute miliary tuberculosis, which resembles in every respect—histological structure, tendencies and presence of bacilli—the acute tuberculosis produced in animals by the inoculation of tuberculous material. It will I think be admitted that the two diseases are identical, and as they are identical their cause must be the same, viz. the tubercle bacillus. We may therefore say definitely that the tubercle bacillus is the cause of acute tuberculosis and that scrofulous glands, degenerated (strumous) synovial membranes of joints, phthisical lungs, (in short, all those materials obtained from man which, inoculated into animals produce acute tuberculosis), contain in them bodies (bacilli) which, if they entered the circulation in sufficient numbers, would give rise to acute tuberculosis. It has been demonstrated by several observers that probably in all cases of acute tuberculosis a place can be found where these bacilli get into the circulation. Ponfick was the first to touch this question, and he showed that in acute tuberculosis, especially in children, tuberculous changes which he regarded as the source of the infection of the blood, were found in the thoracic duct. These however are only found in a small number of cases of acute miliary tuberculosis. Weigert and Mügge found that in many cases of acute tuberculosis there were tubercular masses in the walls of the pulmonary veins and these Weigert considers as

the primary source of infection. It is not however always the pulmonary veins which are the seat of this infection. Weigert has found that the walls of the innominate vein have become affected from the bronchial glands, and he has also found masses in the splenic vein, &c. Koch's case of acute miliary tuberculosis of the bronchial gland in which the wall of an artery was involved (No. 12 p. 292) also in his opinion shows a third mode in which this general infection may take place.

The relation of the tubercle bacilli to localised tuberculous processes in man (phthisis, scrofulous diseases of the glands, joints, &c.) is much more difficult to understand. The only one of these processes which I have had the opportunity of studying fully is phthisis, and in order to make clear the conception which I have formed of the relation of bacilli to this disease, I must point out one or two facts which I have observed as to the mode of distribution of these organisms in the tissue and their relation to the histological elements of the tissue.

In studying artificial tuberculosis in animals we have now a better opportunity of deciding what a tubercle is, and what parts of a tubercle may be looked on as essential histological elements, for we have the cause of the nodule—the bacillus—before us, and can study its precise relation to the nodule. I have, therefore, taken advantage of my opportunities to study this matter, and I have directed my attention to tubercles in the lungs and liver. I have not had time as yet to study the origin of the cells in tubercle of other organs, but the structure of tubercle and the relation of bacilli to the elements of the nodule is the same in all the organs which I have examined (lung, liver, spleen, kidney, lymphatic gland, and intestine). The description which I give here applies specially to the lungs.

Two distinct structures have been described, and may be readily recognised in a tuberculous lung, viz., nodules of lymphatic tissue in close proximity to the vessels and bronchi, and nodules which are largely made up of epithelioid cells. If a case of commencing tuberculosis of the lung be examined, it will be found that bacilli are only present in the latter nodules; and, indeed, it is rare, even in the later stages, to find them in the former, and in that case epithelioid cells will be found as well. The bacillus being the cause of this disease, only the

nodules containing epithelioid cells are tubercles. The others are, I believe, merely the normal lymphatic tissue which exists in considerable quantity, more especially in the guinea-pig's lung, probably hypertrophied by absorption of irritating materials, the products of the growth of the bacillus.

Not only are the bacilli present solely in nodules containing epithelioid cells, but it will be found, on careful investigation, that they are only present in or among the epithelioid cells themselves. Of course I speak here of young tubercles, and of tubercles where the bacilli are in moderate or considerable numbers. Where there are enormous masses of bacilli they may be found in the outer part of the tubercle, though even in this case they are generally confined, in the first instance, to the epithelioid tissue. Where the bacilli are few, one need only look for them in epithelioid cells. (Plate II., Figs. 12 and 13.)

Surrounding the epithelioid cells, which are always in the central portion of the tubercle and make up the greater part of it (though after a time leucocytes penetrate among them), we have what I consider to be simply inflammatory tissue, but what is sometimes spoken of as lymphatic tissue. I have not been able to convince myself of the existence of the so-called reticulum of tubercle, certainly not of the existence of a delicate reticulum resembling that found in lymphatic glands. It seems to me that the appearance of a recticulum is explicable as follows :—The inflammation set up by the development of the epithelioid cells is in the first instance but slight. The normal fibrous tissue surrounding the epithelioid mass becomes infiltrated with leucocytes, or it may be that new fibrous tissue is in some cases formed, and the fibrous tissue containing leucocytes presents a reticular appearance somewhat resembling coarse lymphatic tissue.

As the tubercle gets older, it is found that the epithelioid cells at the centre undergo cheesy degeneration, and they can only be seen, if present at all, at the margin. In this case the bacilli are present in the caseous mass, but they are best seen at the margin of it where epithelioid cells still exist, though they may also then be found penetrating into the inflammatory tissue.

In tubercle we also find giant cells in which bacilli are generally present, sometimes in considerable numbers. These

giant cells I have distinctly traced to epithelioid cells, especially to epithelioid cells containing bacilli; for where several bacilli are present in cells all gradations may be found between the single nucleated cell and the multinucleated giant cell. In inoculated tubercle, giant cells of large size are not common, I presume because time is required for their formation, while in inoculated tubercle the process is too rapid and degeneration occurs early.

As to the origin of these epithelioid cells I can only speak from a study of the process in the lungs and the liver. In the lungs I am satisfied that the great majority are derived from the alveolar epithelium. The bacilli escape from the blood-vessels or lymphatics and get into the alveolar epithelium, where they grow and cause multiplication of the epithelial cells, till the alveolus becomes completely filled with these cells and infiltrated leucocytes. Around this mass the walls of the alveolus become inflamed and thickened, and form the granulation tissue surrounding the epithelioid mass (Plate II., Figs. 12 and 13). The idea of some is that the epithelioid cells are derived from leucocytes, but in cases where the epithelioid mass has become caseous, bacilli may be present in the tissue around which is granulation, not epithelial tissue, and these granulation cells do not become epithelioid. It is hardly conceivable that these bodies possess the property of converting leucocytes into epithelioid cells; it is surely much more likely that they grow by preference in epithelioid cells already existing and lead to their multiplication. I will not assert that the epithelioid cells found in tubercles in the lung are always derived from alveolar epithelium; they may also be derived from the endothelium of the blood and lymphatic vessels. Nevertheless I believe that the great majority are alveolar cells. In the case where Koch injected bacilli into the veins of a rabbit in my presence, I found that many of the masses of epithelioid cells were undoubtedly derived from the alveolar epithelium, but there were others in which I could not trace this mode of origin, and in which they must have been derived from the lymphatic or blood-vessels. In lymphatic glands their source must be from one of the latter, I believe from the lymphatic endothelium. Schüppel is inclined to regard them in this case as generally derived from

blood-vessels, but this I cannot admit, for if the bacilli were present in the blood-vessels there seems no reason why the disease should be arrested in the glands even for a time, as, however, is found to be the case.

In the liver I have frequently found the bacilli in liver cells at the margins of the tubercles, and where tubercles were commencing to form; and even where the tubercles are older, direct continuity between the epithelioid cells and the liver cells can frequently be traced, especially by the use of Ehrlich's method of staining with hæmatoxylin, rubin s. and orange. I have also seen the liver cells, in which bacilli were present, with two nuclei, and apparently giant-cells are often formed by multiplication of the nuclei in liver-cells or by fusion of liver-cells. In the latter case regular tubes may be formed, which on transverse section would appear as Langhans' cells with a circular arrangement of nuclei around the wall.

The origin of epithelioid cells from the endothelium of blood-vessels has been described by Laulanié in connection with the strongylus vasorum previously mentioned, and I saw it distinctly in the specimens which he showed me.

Giant cells are epithelioid cells from any of the sources mentioned which have grown rapidly, apparently as the result of the presence of bacilli in them. In the lung they are generally derived, like the ordinary epithelioid cells, from the alveolar epithelium. I have seen this distinctly, and in one case I found nothing in an alveolus but one large giant cell containing a number of bacilli (Plate I., Fig. 7). This cell may have arisen from fusion of several cells or from the disappearance of the other epithelioid cells. The fact that giant cells generally contain a large quantity of pigment seems also to indicate that they were originally alveolar cells.

Tubercle has been regarded as a non-vascular tissue due to obliteration of the vessels in the centre. This is not the case, for, from the facts just mentioned, it will be seen that vessels were never present in the centre. At the same time, however, the accumulation of epithelial cells leads to pressure on the vessels outside, and blocks them. When this accumulation of epithelium has gone on to a certain extent caseous degeneration of these cells sets in, and thus the centre of the tubercle becomes

converted into a cheesy mass. Where the process is rapid the giant cells also disappear, as is generally the case in tuberculosis artificially induced. When the accumulation of epithelioid cells is great, the pressure on the surrounding tissue leads to its atrophy and degeneration, and thus neighbouring cheesy masses come to communicate with each other. Where the process is slow and the accumulation of epithelioid cells not very great, the granulation tissue surrounding them may become organised into fibrous tissue more or less perfect. It may be also that the caseous matter becomes absorbed, leaving only the giant cells in the midst of the fibrous tissue, or it may be, as in the case cited before, that all the cells of an alveolus have become fused together to form a giant cell, or the cells disappear except the giant cell, and thus, in any case, we have a giant cell left in the midst of fibrous tissue. As this giant cell contained bacilli in the first instance it may still contain them, and thus is explained the fact pointed out by Koch, that bacilli are very frequently present in giant cells, and that, in many cases where there is only fibrous tissue and giant cells, the latter are the only parts where one finds bacilli. What the further fate of these giant cells may be is difficult to say, but in one case I have seen distinct indications as if they were going to develop into blood-vessels (Plate II., Fig. 8).

From these facts it is evident that if we want a typical histological element for tubercle we must take the epithelioid cells, because they are always present in tubercles, and because the bacilli are always found in them in the first instance. They will not, however, be always found, as caseation sets in early and destroys them. In that case, if the process is slow, giant cells, may still be seen, the only remnants of the original epithelioid cells.

The structural definition of a tubercle must therefore run somewhat as follows: A nodule, composed of a central mass consisting in the main of epithelioid cells, or in its place a cheesy mass, surrounded by more or less inflammatory tissue, with or without the presence of giant cells. The absolute diagnostic mark is, however, the presence of the tubercle bacillus. A group of granulation cells without epithelioid cells, or without cheesy matter or giant cells to indicate the previous presence of

epithelioid elements, is not a tubercle. On the other hand I know of no morbid structure except tubercle which contains the same histological elements, arranged in the same way, and possessing the same tendencies. It is not, however, always necessary for a tubercle to be a nodule. If there are plenty of epithelial cells, or if it occurs where there are no pouches as there are in the lung, it may be diffuse (see remarks on appearance in Koch's *Dog's Liver*, p. 278). In any case, as I have just said, the absolute diagnostic mark of a tuberculous process is the presence of tubercle bacilli.

With regard to phthisis, the divisions and subdivisions of this disease are too numerous and varied to be of importance for the present question. We may take the two extremes—the rapid phthisis or caseous pneumonia, and the chronic or fibroid phthisis. If we examine the former cases we find alveoli distended with caseous material, or, in parts where the process is less advanced, with epithelioid cells, and the trabeculæ surrounding these thickened and converted into inflammatory tissue. In this case the bacilli are found in moderate or even in considerable numbers, in the caseous material and epithelioid cells filling the alveoli. By and by we find that the walls of the alveoli disappear, and thus irregular cavities are formed, containing caseous material surrounded by epithelioid cells and inflammatory tissue. In this case the bacilli are most numerous, and sometimes in enormous masses, at the free margin of the cheesy material (Plate I., Fig. 3). They are also present, though not generally so numerous, in the epithelioid cells at the line of junction of the caseous mass with the surrounding tissue (Plate II., Fig. 11). It does not follow that in every section one will find many bacilli; in some they are very few, but the examination of a considerable number of sections will generally result in the discovery of considerable numbers at the free margin of the caseous material or in alveoli.

In fibroid phthisis the bacilli are, as a rule, extremely few, but here and there, if a cavity exists, or in the centre of a caseous mass (Plate I., Fig. 2), one may find considerable numbers. They may also be found in the giant cells, which are generally pretty numerous among the fibrous tissue, but this is very rare. As a rule, with the exceptional cases I have just mentioned,

bacilli are extremely few in fibroid phthisis; but nevertheless, if a sufficient number of sections be carefully examined, one or two will be found here and there at the margin of, or in, the caseous masses.

The following is what the foregoing facts lead me to suppose to be the sequence of events in phthisis. The tubercle bacilli which reach the lung[1] by inhalation develop in the epithelial cells lining an alveolus, this alveolus becomes filled with cells, neighbouring alveoli become infected, and the same process goes on in them. The further result will depend on the number and rapidity of growth of the bacilli, and on whether the patient is a good soil for their development. If they develop well, we have caseous pneumonia, if they grow slowly and with difficulty, we have fibroid phthisis. In the former case the alveoli become distended early with epithelioid cells, this leads to inflammation of the walls of the alveoli, the cells soon undergo cheesy degeneration, and the pressure of the masses leads to atrophy or sloughing of the walls of the alveoli. (In the latter case elastic tissue will be found in the sputum.) Infection of neighbouring parts of the lung occurs both by continuity, and also by partial coughing up and re-inhalation of the bacilli into other parts of the lung. In this rapid phthisis, fibrous formation around the alveoli only takes place imperfectly, and the lung rapidly breaks down.

In the case of fibroid phthisis the bacilli are few, and grow only with difficulty. Thus fibrous formation occurs extensively, and giant-cells are caught in this fibrous tissue in the manner formerly described. Nevertheless, in parts the process may be more rapid, and there cheesy masses are formed which may lead to breaking down of the lung and the formation of cavities.

On this view we have an explanation of several facts. First, we have the rarity of acute general tuberculosis in connection with phthisis, even though bacilli are present in the lungs. One

[1] I do not imply that bacilli when inhaled must grow in the epithelial cells; they may pass on and be caught in the bronchial glands, or they may not grow at all I only refer to what occurs in cases where phthisis develops. I do not think that the development of phthisis is merely a question of soil, but it seems to me that the lung must in addition be prepared, so to speak, for the reception of the bacillus, as may be the case if congestion or slight inflammation be present at the time of the inhalation of the organism.

reason of this is probably that the bacilli can hardly be said to enter the body: they are separated from the circulation by the layer of granulation and fibrous tissue. This is a fact which can be readily observed.

Secondly, we have the explanation of their presence in the sputum even before physical signs are marked or indeed have become evident at all. It is now stated that the number of bacilli in the sputum is a means of forming a prognosis as to the rapidity of the disease. According to the views I have just stated this would be a very likely thing, for the presence of large numbers in the sputum would indicate either an affection of numerous alveoli, or a large amount of caseous material, *i.e.* extensive affection of one part of the lung, and hence rapid death.

It will also be seen that the relation which was early maintained between cheesy pneumonia and what is ordinarily understood by tuberculosis of the lung, a relation denied by some, does in fact exist, and that the difference between the two lies in the mode of the spread of the disease. Caseous pneumonia arises from inhalation of the virus into the alveoli, while miliary tuberculosis and the eruption of nodules over the lung in acute phthisis are due to infection by the blood-vessels in the first case, or by the lymph channels in the second. No doubt, however, the second case is also sometimes due to inhalation of tuberculous sputum into other parts of the lung during some mishap in coughing.

Against the view that phthisis is due to these bacilli might be urged the fact that the bacilli found in the lung after death are often very few in number. This fact is undoubted in some cases, and it is certainly difficult to understand how a few bacilli can set up such extensive changes. Not only are few bacilli found in phthisis, but there are also few in the tubercles in acute tuberculosis and in other tuberculous processes in man. Indeed, in tuberculous material in man, except in the case of the glands, as the bronchial and mesenteric glands, bacilli are few. Now, if the notes which I have given in the experimental part be carefully gone over, it will be seen that in all cases (with one exception) where material obtained from man was employed for inoculation I have noted the number of bacilli as few and moderate—not numerous; while in the case of

inoculation of perlsucht the bacilli are, as a rule, numerous. (In the glands, however, they are numerous in both cases.) This did not strike me for a considerable time, but after I noted it I went over the cases again and found it to be the fact. Indeed, in some instances, it was surprising what an extensive tuberculous process was set up in guinea-pigs and rabbits with very few bacilli in the tubercles, even where cultivations of bacilli had been inoculated. The difference in the number of the organisms present in the tubercles produced in rabbits by inoculation of bacilli from man and in those from bovine tuberculosis has been almost constant in my experiments; but I am not inclined to lay it down as an axiom, for in two cases inoculated by Koch with bacilli from man I find them very numerous in the tubercles produced. (See No. 9, p. 291 and No. 5, p. 279.) In the first of these cases the sections I made were thick, for I used a machine to which I was not accustomed, and that may to some extent account for the large numbers found, but in the last case I used Williams's machine and the sections were thin. This is therefore a point which requires further investigation, but I must say my results are very striking. Leaving out of account, however, this possible difference in energy between the bacilli in man and in perlsucht, which, were it proved, would be most important in connection with the question at issue, we have, nevertheless, the fact that very extensive tuberculous processes may be found in animals with only very few bacilli, and that in cases where we are sure that the bacillus was the only agency at work.

The views which I have just put forward with regard to phthisis are not at all opposed to the fact that inoculation of rabbits, &c., is generally followed by general tuberculosis, for the mode of infection differs in the two cases. Where tuberculosis is artificially produced, the bacilli are placed at once in direct communication with the lymphatic and blood vascular systems, while in the natural infection of the lungs by inhalation the bacilli grow outside the body and a barrier of granulation tissue is formed which prevents their entrance into the vessels. This is, however, not the only reason of the difference between the two; there must also be a special predisposition on the part of rabbits, &c., to general tuberculosis. For when the inocula-

tion is made into the eye, the process is at first local and afterwards becomes general. This may, indeed, be due to some anatomical peculiarity of the part, but I think that it is most likely in the main a question of soil, or what is termed predisposition. That certain organs and tissues are better soil than others for the growth of parasites is well known, and can easily be shown with regard to tuberculosis; the apices of the lungs are better soil than the bases, epithelioid cells are better soil for the bacillus than granulation cells or cheesy matter, and so on. Now, it does not necessarily follow that the quality of the soil is the same in man and animals. In cattle the bacilli prefer the surfaces of the serous membranes to an extent which they only very rarely do in man. In man, again, the pia mater is a common seat of tubercles in acute tuberculosis; in rabbits and guinea-pigs the presence of tubercles in the pia mater is, I believe, unknown; certainly they have not been found in my experience. Other facts might be mentioned, but what I have said is sufficient evidence of the difference between tuberculosis in man and rabbits, dependent partly on the mode of infection but chiefly on difference in soil.

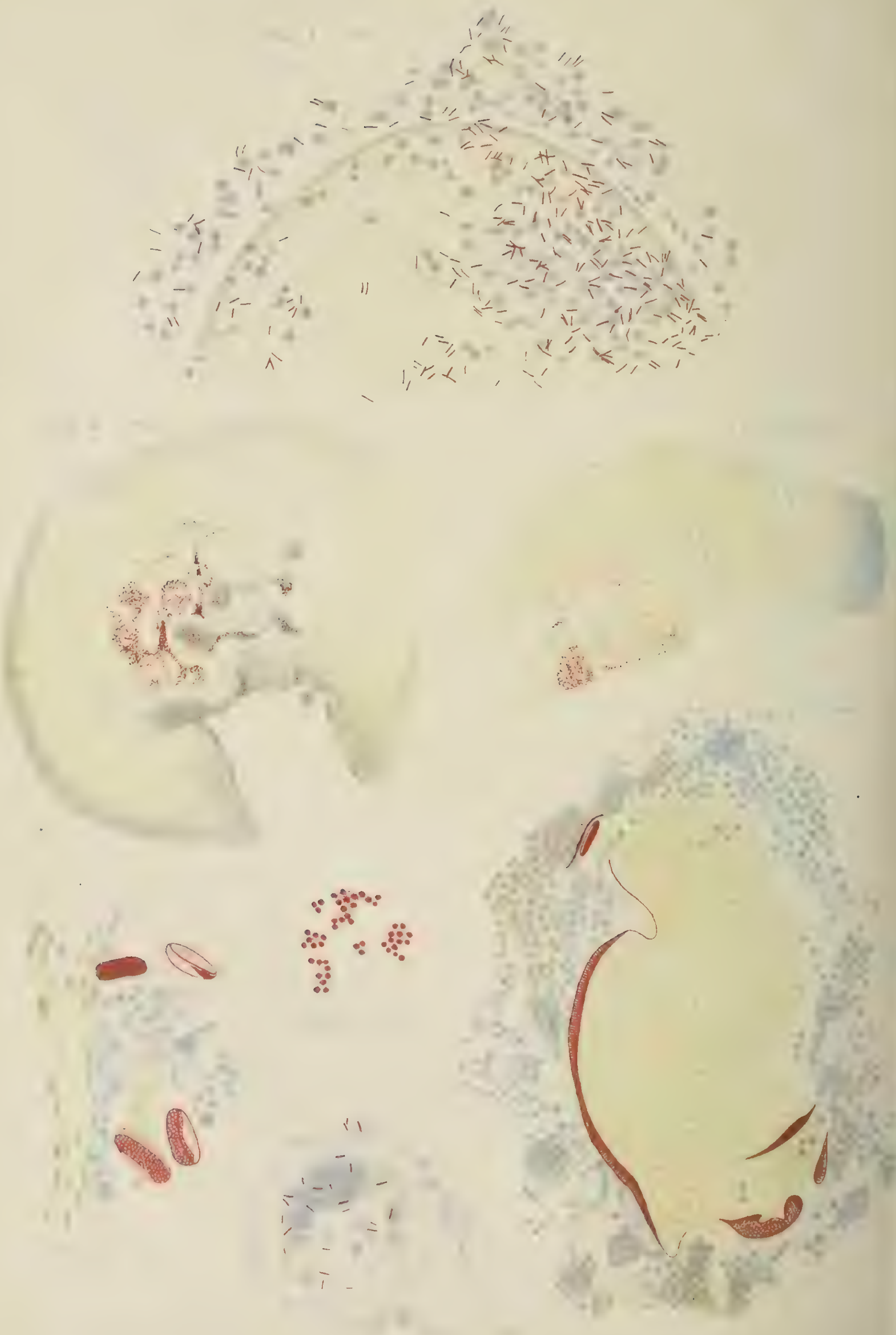
My views as to the nature of phthisis and its production by bacilli receive direct confirmation from the inhalation experiments of Tappeiner (Virchow's *Archiv.*, vol. lxxiv., 1878). He caused dogs to inhale phthisical sputum, which was sprayed into their cage for a certain time once or twice a day and for a number of days in succession. As a result he produced tuberculosis in eleven cases. Now, in seven of these eleven cases the tubercles were limited to the lungs, and I see that he describes some of them as desquamative pneumonia with tubercles. In one case there were a few nodules in the kidneys, and in two a few in the liver and kidneys. In only one case was there a typical acute tuberculosis. Here the method of infection somewhat resembled what we must suppose to be the natural method in phthisis, though in Tappeiner's dogs a much larger quantity was administered, and that much more rapidly than probably occurs in nature. And hence the process was more diffuse and rapid than in man. Nevertheless, acute tuberculosis only occurred in one case, and a few nodules in three others. In seven the process remained limited to the lungs just as I have described in the

case of phthisis. That in a few instances the bacilli did get into the body need not excite surprise, when the amount and continuance of infection are taken into account. That this may also occur in man is well known (see Klebs' list, Virchow's *Archiv.*, vol. xlv.).

A consideration of all the facts has led me to the conclusion that tuberculous processes in the lungs are due to the tubercle bacilli, and, so far as I know, to them only. By a tuberculous process I mean one where there is proliferation of epithelium, caseous degeneration of this proliferated epithelium, and inflammation round about, these changes being progressive. It has been supposed that inhalation of dust of various kinds may give rise to phthisis. That the inhalation of dust will lead to inflammatory changes is very likely, that it may lead to proliferation of epithelium which may subsequently degenerate is possible, but that the process will be progressive and extend beyond the seat of irritation is not probable. That the changes set up by the presence of gritty particles, may, however, prepare the lung and render it a fit soil for the implantation of bacilli is very probable, and in this way a true tuberculous process may supervene, not due to the original gritty substances but to the bacilli which came afterwards. I have only had the opportunity of examining three cases of potter's phthisis and one of miner's phthisis. In the former there was, histologically, a true tuberculous structure, and there the tubercle bacilli were found. In the case which was labelled miner's phthisis, but the details of which I did not obtain, there was fibrous formation, the fibrous tissue being very vascular, and there was no appearance, histologically, of tuberculous structure, nor were any bacilli present.

As to the intestinal ulcerations which often occur in phthisis, and which are supposed to be due to swallowing sputum, I have only examined two cases, and there I found tubercle bacilli in the wall of the ulcer bearing the same relation to epithelioid cells and caseous matter as elsewhere.

As to heredity of tubercle I would call attention to the case of the guinea-pig, which was highly tuberculous and which had an almost fully-developed fœtus in its uterus (Experiment XVIII., p. 289). The fœtus and placenta were healthy and free from tubercles.



It has often been urged that the milk of tuberculous cows is infective. This may be the case when the mammary glands become tuberculous and the mode in which the bacilli might get into the milk is well illustrated by the appearances which I found in the kidney of rabbit No. 1, Experiment XIV. p. 286. There not only were bacilli present in the tubercular mass, they were also found in large numbers in the epithelium of the kidney tubules, and in the interior of the tubules, both in the immediate vicinity of the mass and at some distance from it. I have not yet had an opportunity of examining an early tubercle of the kidney, but, from what I have seen, I think it quite likely that the epithelium of the tubules may, in some cases, be the primary seat of the bacilli in the kidney, just as the alveolar epithelium is in the lung. In that case bacilli would be present in the urine, not merely when there were marked tubercular masses in the kidney, but also where the disease was but slightly advanced, here again resembling the case of the lung. From analogy I suppose that the same is the case with the mammary glands, and that bacilli might be present in the milk even though the disease of the gland is not sufficiently far advanced to be noticeable.

The accompanying Plates were drawn by Mr. Edgar Thurston by means of Beale's neutral tint reflector at the standard distance of ten inches, and are faithful reproductions of the specimens. In all the specimens the bacilli are red.

PLATE I.

Fig. 1.—Section of lung from a rabbit which became tuberculous after being inoculated by Professor Toussaint with the micrococci with which he works. Centre of a tuberculous mass which has become caseous, inclosing a large pulmonary vessel. The tubercle bacilli have penetrated through the wall of this vessel and are seen growing in considerable numbers in its interior. From this point the blood would be constantly supplied with numerous bacilli and thus generalisation of the disease would rapidly occur. × 330. (The optical apparatus employed was Hartnach's No. 7 objective, Zeiss's No. 2 eyepiece, Abbe's condenser.)

Fig. 2.—Section of lung from a case of fibroid phthisis showing a cheesy mass with a group near its centre consisting of numerous bacilli. (See p. 294.) × 100. (Hartnach's No. 4, Zeiss's No. 2 eyepiece, Abbe's condenser.)

Fig. 3.—Section of lung from a case of potter's phthisis showing the margin

of a cavity with masses of bacilli at the edge. × 100. (Hartnach's No. 4, Zeiss's No. 2 eyepiece, Abbe's condenser.)

FIG. 4.—The structure in the lung referred to at p. 298. × 200. (Hartnach's No. 5, Zeiss's No. 2 eyepiece, Abbe's condenser.)

FIG. 5.—Toussaint's micrococci. × 950. Zeiss's $\frac{1}{12}$th oil immersion, No. 4 eyepiece, Abbe's condenser.)

FIG. 6.—Psorospermiæ from the liver. Experiment I., No. 2. × 330. (Hartnach's No. 7, Zeiss's No. 2 eyepiece, Abbe's condenser.)

FIG. 7.—Alveolus filled by a giant cell containing several bacilli. Experiment XIV., No. 3. × 330. (Hartnach's No. 7, Zeiss's No. 2 eyepiece, Abbe's condenser.)

PLATE II.

FIG. 8.—Section of fibrous part of lung from which Fig. 2, Plate I. was taken, showing giant cells, one of which seems to be forming a blood-vessel. No bacilli are present among the fibrous tissue or in the giant cells. The advantages of flooding the field with light by means of a condenser properly employed is well seen, for nothing but the stained bodies is visible, though in reality the main mass of the field consisted of dense fibrous tissue. × 330. (Hartnach's No. 7, Zeiss's No. 2 eyepiece, Abbe's condenser.)

FIG. 9.—Section of kidney at margin of tuberculous mass showing kidney tubule containing a mass of caseous material with numerous bacilli. Experiment XIV., No. 1. × 330. (Hartnach's No. 7, Zeiss's No. 2 eyepiece, Abbe's condenser.)

FIG. 10.—Tubercle bacilli. × 2,350. (Powell and Lealand's $\frac{1}{25}$th oil objective N.A. 1·38.)

FIG. 11.—Wall of cavity in case of phthisis showing the epithelioid tissue between the caseous mass and the inflammatory tissue. Bacilli in the epithelioid cells resembling somewhat the appearance of the bacilli in leprosy as regards their arrangement. × 330. (Hartnach's No. 7 objective, Zeiss's No. 2 eyepiece, Abbe's condenser.)

FIG. 12.—Tubercles in the lung showing the alveoli full of caseous material and bacilli. × 330. (Hartnach's No. 7, Zeiss's No. 2 eyepiece, Abbe's condenser.)

FIG. 13.—Tubercle in the lung showing epithelioid cells in alveolus surrounded by thickened wall of the alveolus. Three bacilli in the centre of the epithelioid mass. × 330. (Hartnach's No. 7 objective, Zeiss's No. 2 eyepiece, Abbe's condenser.)

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Original Communications.

THE VALUE OF SOME NITRIC, NITROUS, AND NITRO-COMPOUNDS IN ANGINA PECTORIS.

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In a recent number of this Journal[1] I gave an account of the successful employment of a simple metallic nitrite, as nitrite of sodium, in the treatment of angina pectoris, and pointed out the advantages obtained by its use as compared with the compounds hitherto employed in the treatment of angina, viz., nitrite of amyl and nitro-glycerine. Since then, I have, in other cases, obtained confirmation of its value.

It will be remembered that the case described exhibited in an unusually well-marked degree the symptoms of angina pectoris, which rendered the successful trial of the nitrite of sodium all the more satisfactory. For the same reason I considered it a case well-adapted for the trial of a few other nitrites and some compounds more or less closely related to the nitrites, partly with the object of establishing the correctness of my supposition that the nitrous acid was the remedial part, and partly for the purpose of ascertaining in what form the nitrous acid could be best administered. These trials were made at intervals during

[1] March, 1883, p. 179.

the treatment of the case, principally during January and at the beginning of February, when, although the angina was becoming less severe, it was still sufficiently marked to fairly test the remedial power of the substances employed. It is almost superfluous to mention that the administration of the nitrite of sodium was discontinued whilst these trials were being made, and that by desisting from the use of any remedy for a short interval—half a day, or longer if necessary—the angina was ascertained to be still present. At the time these experiments were made the attacks of angina, provided no nitrite was administered, came on at tolerably fixed periods throughout the twenty-four hours—invariably shortly after rising and whilst dressing, occasionally after breakfast, usually after dinner, occasionally in the course of the evening, and, almost without fail, once or twice during the night; besides other attacks at any period of the day when anything occurred to excite him, or when he worked more actively or walked too quickly.

The substances, whose therapeutic value was tested, may be divided into six groups:—(1) Ethereal nitrites, other than nitrite of amyl; (2) metallic nitrates; (3) ethereal nitrates of the type of nitrate of ethyl; (4) ethereal nitrates of the type of nitro-glycerine; (5) nitro substitution-compounds; (6) compounds of amyl, other than the nitrite.

The representatives of these various groups actually used were of:—(1) nitrite of methyl [$CH_3.O.NO$], and nitrite of ethyl [$C_2H_5.O.NO$]; (2) nitrate of sodium [$Na.O.NO_2$]; (3) nitrate of ethyl [$C_2H_5.O.NO_2$]; (4) pyroxylin [$C_6H_5.5\text{-}n.(HO).n(O.NO_2)$]; (5) nitro-benzol [$C_6H_5.NO_2$], and picric acid or trinitrophenol [$C_6H_2.3(NO_2).OH$]; (6) chloride of amyl [$C_5H_{11}.Cl$], and nitrite of amyl [$C_5H_{11}.NO_3$].

It was to be expected, if combinations of nitrous acid were alone active in the treatment of angina, that only the representatives of group (1), as also those of group (4), which probably yield nitrous acid by decomposition in contact with an alkaline fluid, as the blood or intestinal secretions, would prove remedial; whilst the representatives of group (2) and of (3), the latter of which, unlike (4), are not decomposed with the formation of nitrous acid, and the representatives of group (6), would prove inactive, as also those of group (5), which, at first sight, bear a

very close resemblance to those of group (1), but which differ widely from the latter in their intra-molecular arrangement. This is more clearly brought out by a comparison of the formulæ of the nitrite and the nitro-compound of the same alcoholic radical, as of ethyl:—$C_2H_5.O - N = O$, nitrite of ethyl; $C_2H_5.N\left\langle\begin{matrix}O\\ |\\ O\end{matrix}\right.$ or $C_2H_5.N\begin{matrix}{}^{/\!\!/}O\\ \\ {}_{\diagdown}O\end{matrix}$, nitro-ethane. The empirical formula is the same in both cases, but with that the resemblance ceases, as they present different physical characters and yield different decomposition-products and produce different effects on the organism.

Effect of Group (1)—*Ethereal Nitrites.*[1]—It was to be expected that the members of this group would behave much in the same manner as nitrite of sodium was found to do, particularly if the nitrous acid were the principal active part of each. The results of a few experiments with them realised my expectation. For the ethereal salts proved nearly as active as the metallic salt in preventing or arresting the anginal attack. This, however, was not at first quite apparent in the case of the nitrite of ethyl owing to its solution, although obtained from a London firm of high repute, containing, as I afterwards discovered, less than one-fifteenth of the nitrite it was represented to contain. The methylic ether is gaseous even below 0°C., and the ethylic compound, although less volatile than the other, is also gaseous if the temperature be higher than 16° C.; accordingly, they are always supplied in solution in alcohol; and it was in this form that I obtained and used them, the nitrite of methyl as a 10 per cent. solution, and the nitrite of ethyl as a 25 per cent. solution. The latter solution, however, as mentioned, really contained little more than $1\frac{1}{2}$ per cent. of the ether.

Before administering either of them to the patient I tried their physiological effect on myself, and found that the solution of the nitrite of ethyl up to a dose of twenty minims produced no perceptible effect—no increase of the pulse-rate, no throbbing, no giddiness, no flushing. It was otherwise with the

[1] The physiological action of nitrite of ethyl has been investigated by Dr. Benj. Richardson, who came to the conclusion that it closely resembles nitrite of amyl in its action. *Brit. and For. Med.-Chir. Review*, vol. xl. 1867, p. 259.

solution of the nitrite of methyl, five minims of which increased the rate of the pulse, in the course of a minute and a half, from 66 to 72, at which it remained for two minutes, and in a minute more fell to 68. I then took other ten minims, with the effect of raising the pulse within two minutes to 70, but a minute afterwards it fell to 67. The character of the pulse, in so far as it could be recognised by the finger, did not seem to be much altered. Throughout the immediate action of the nitrite there was a slight feeling of flushing in the face, and of warmth all over the body, but there was no throbbing. Fifteen minutes after the administration of the nitrite there was a small eructation of gas tasting of nitrous acid, showing that the ether was being decomposed by the acid of the stomach.

Given to the patient, the solution of nitrite of ethyl failed to have any effect until a dose so large as sixty to seventy minims had been taken, and then it acted like the nitrite of sodium, although not so powerfully. The dose was evidently not large enough. As the supply of the solution failed me at this time, no opportunity was given the patient of trying a still larger dose.

The nitrite of methyl solution acted more promptly on account of its containing more nitrous acid. In a dose of twenty minims it dulled the pain, and in a dose of thirty minims effectually removed or prevented it.

From these experiments it appears that the nitrites derived from the primary alcohols act towards angina pectoris much in the same manner and in the same dose as nitrite of sodium. Although ethereal, they do not possess the great activity of nitroglycerine. They possess no advantages over the alkaline nitrites, and, certainly, as regards expense and stability, are greatly beneath them.

Effect of Group (2)—*Metallic Nitrates.*—In my paper on the nitrite of sodium I stated that previous to publication I found that the salt which was being taken by the patient contained only 33 per cent. of nitrite, the remainder being nitrate. It is a natural suggestion that the nitrate may have been the active part of the preparation, or, at least, was equally active with the nitrite. I, however, found as I had fully anticipated, that pure nitrate of soda was perfectly without effect when administered alone, even in so large a dose as ten or twenty grains.

Effect of Group (3)—*Ethereal Nitrates of the Type of Nitrate of Ethyl.*—The nitrate of ethyl was, like the nitrate of soda, quite without action on the attack of angina. And it is, therefore, probable that all other nitrates, whether metallic or ethereal, unless within the body they become decomposed with the formation of nitrous acid, will be found to be equally without action.

Effect of Group (4)—*Ethereal Nitrates of the Type of Nitro-Glycerine.*—Nitro-glycerine, or the nitrate of glyceryl [C_3H_5. $3(O.NO_2)$], differs from the ethereal nitrates of the previous group in yielding up a large proportion of its nitric acid in the form of nitrous acid or a nitrite when treated with an alkali. This is a point which I have recently determined, and it is highly interesting as affording an explanation of the apparently anomalous action of the nitrate of glyceryl, the ether becoming decomposed by the alkaline blood. I shall not at present say more on this point, as I hope at an early period to be able to present an exact statement of the nature of this decomposition. But nitro-glycerine does not stand alone in the nature of its decomposition. Several other ethereal nitrates, somewhat allied to nitro-glycerine, behave, in all probability, in a similar manner, as nitro-saccharose, nitro-amylum, and nitro-cellulose or pyroxylin (gun-cotton). The last of these, as I have ascertained, undergoes certainly a similar decomposition. Now as these bodies differ from nitro-glycerine in being solid and non-volatile, and are very insoluble unless in alkaline menstrua, I thought that, if they were given internally in cases of angina pectoris, they would become slowly decomposed by the alkaline fluid of the intestines, and thus maintain a small but constant supply of nitrous acid or nitrites to the blood, which might continue for even a day after the administration of the substance. Accordingly I made trial of pyroxylin, as obtained from collodion by adding water. I took it myself, in the first instance, to the extent of ten grains, but I could not say that I afterwards experienced any effect that could reasonably be attributed to it. I then gave the same dose on two separate occasions to the patient, and he likewise experienced no effect from it, as the angina was in no degree relieved, but recurred as frequently and as strongly as before. I concluded that the nitro-cellulose is too insoluble in the weak alkaline juices of the

alimentary canal to be of any service in the treatment of angina, as the little nitrous acid that may be set free during its digestion is reduced or oxidised by the contents of the canal before its absorption is permitted. I am not, however, without hope that among the compounds allied to nitro-cellulose one may be found which will fulfil the desired conditions of being decomposed in the alimentary canal slowly, yet quickly enough to permit of a continuous supply of nitrous acid to the blood over an extended period. The main fault of the nitrous compounds at present in use, even of nitrite of sodium—the best of them in this respect—is that their action on the body in warding off an attack of angina does not extend for more than for four or five hours after their administration. Hence they do not prevent the occurrence of attacks during the night, and cannot secure a night's rest free from the possibility of interruption.

Effect of Group (5)—*Nitro-substitution Compounds.*—It appeared interesting, in respect of the close relationship of these compounds to the corresponding nitrites—both possessing the same empirical formula—to ascertain whether they possessed the same action as the nitrites in angina pectoris.[1] As representatives of the group, I used nitro-benzol and picric acid (tri-nitrophenol). My experiments might have been more conclusive had I employed similar compounds derived from the primary alcohols, but these others were not at hand and could not readily be procured. As it is, I think, we are quite entitled to believe that the nitro-compounds used will, in so far as the radical, NO_2, is concerned, behave towards the organism like all other nitro-substitution compounds.

As nitro-benzol is volatile, I administered it by inhalation; the picric acid I of course gave by the mouth. As in the case of other compounds tried, whose action is not well-known, I observed

[1] The physiological action of the nitro-compounds, so far as it has been investigated, does not resemble that of the nitrites. The nitro-compounds have much less effect on the circulation than the nitrites. For experiments on the physiological action of nitro-methane, -ethane, and -pentane, *vide* Schadow, *Arch. f. exper. Path. u. Pharmak.* Bd. vi. S. 194 (1876), and Filehne, *Centralb. f. med. Wiss.* 1876, No. 49, S. 867. Nitro-benzol, *vide* Lewin, *Virch. Arch.* Bd. lxxvi. S. 443; Starkow, *Arch. f. path. Anat.* Bd. lii. S. 464; Filehne, *Sitzb. d. Erlang. phys. med. Gesellschf.* Dec. 10, 1877, and others. Picric acid, *vide* Husemann's *Toxicologie*, 1862, s. 726; Erb. *Pikrinsäure*, Wurzburg, 1865.

the effects of both compounds on myself before giving them to the patient. I found that the inhalation of twenty drops of nitro-benzol did not perceptibly affect the rate of the pulse or the condition of the circulation generally. The patient gave it twice a trial immediately previous to the onset of an attack, inhaling as much on one occasion as eighty to one hundred drops, yet completely without effect. Picric acid, which in a dose of five grains I found to have little effect on myself, was given to the patient in pills containing three grains each, ordering that one should be taken at bed-time, another two hours before rising, a third after rising, another before dinner, a fifth in the course of the evening, and a sixth at bed-time. Yet, though he adhered strictly to these directions, the pain came on at its usual periods and with its intensity in no wise diminished. The acid imparted a distinct yellow colour to the urine, and the patient exhibited a slight jaundiced appearance for two days afterwards.

Nitro-substitution compounds have, therefore, apparently no effect on angina pectoris. This is an interesting therapeutical proof of the correctness of the distinction that chemists have drawn between them and nitrites. Unlike nitrites, they are not decomposed by acids to form nitrous acid, nor by alkalies to form nitrites.

Effect of Group (6)—*Compounds of Amyl other than the Nitrite.*—More interest attaches to the action of this group than, perhaps, to that of any other, on account of the amyl having been long regarded as the active part of nitrite of amyl in the treatment of angina pectoris. Dr. B. Richardson, who made several experiments on the physiological action of this and other compounds of amyl, when asked at a meeting of the British Association[1] what was the active constituent of the nitrite of amyl—the nitrous acid or the amyl—gave it, as his decided opinion, that to the latter mainly was to be ascribed the activity of the ether; and this has practically remained until now the opinion of the profession generally. My experience of it goes to show that Dr. Richardson, although not altogether correct in his surmise, is yet not wholly wrong. If the amyl be the active part, then it is to be expected that other ethereal compounds of amyl will produce the same action as the nitrite;

[1] *Transactions Brit. Assoc.* 1865, p. 272, and 1866, p. 172.

for example, the nitrate and the chloride, both of them volatile liquids, possessing similar physical characters to those of the nitrite.

As usual, I made the first experiment on myself, inhaling the nitrate, which was previously ascertained to contain no trace of the nitrite, and to be otherwise a good and pure preparation. As no experiments have been as yet recorded of the action of the nitrate of amyl, I think it advisable to give the protocol of my experiment with it :—

11.8	A.M.	Pulse, 66.
11.10	,,	,, 68.
11.12	,,	,, 66.
11.13	,,	Commenced to inhale ten drops of nitrate of amyl on a handkerchief.
11.14	,,	Added ten drops more.
11.16	,,	Inhalation stopped.
11.16½	,,	Pulse, 96. Feeling of fulness in the head, slight throbbing in the sides of the head, especially above and behind the ears, singing in the ears, very slight sense of flushing of the face, slight giddiness ; respiration slow and deep.
11.18	,,	Pulse, 88.
11.19	,,	,, 88.
11.21	,,	,, 84. Sensations described are now gradually disappearing.
11.23	,,	,, 76.
11.25	,,	,, 68—72, variable.
11.30	,,	,, 72. Variable, but fairly strong. Still a feeling of fulness about head and ears.
11.31	,,	,, 84.
11.35	,,	,, 68.
11.45	,,	,, 68. Headache and slight nausea. The headache continued more or less for the remainder of the day.

I have several times, both before and since making this experiment, inhaled the nitrite of amyl, but I have never found it in its usual dose of five or six drops to so greatly increase the rate of the pulse as the nitrate did, and I have always observed that it occasioned a much more distinct flushing of the skin and much more powerful throbbing in the arteries.

The patient began with the inhalation of the chloride, and used it, for the first time, to relieve him of a severe attack of pain which had come on during the night. He inhaled over thirty to forty drops, and it relieved the pain slowly but tolerably effectually. He again tried it in the morning on getting up, when another attack of pain had commenced, and with an equally good result. A single trial of the nitrate on another

occasion had the same effect. After inhalation, he felt a sensation of darkness over his eyes, and giddiness and throbbing in his head and limbs; the accompanying headache did not continue long. He said the effect was much like that of the nitrite of amyl with the exception that it was not so quickly produced, and that he required to use a very much larger dose.

Amyl would appear, therefore, to possess an action on the circulation and angina pectoris somewhat similar to that of nitrous acid, so that the benefit derived from nitrite of amyl in angina is not wholly due to the nitrous acid, as the action of the nitrite of sodium would incline us to believe, but is in part caused by the amyl.

These are the results of my experiments with various nitrogenous compounds in the treatment of angina pectoris. It may be wondered at that for six or seven weeks a patient was found so willing to submit himself to all the trouble involved. The trouble was not, however, so great as might at first sight appear; for angina pectoris is a disease which yields either at once or not to the remedy tried. One or two trials of the drug in a sufficient dose are quite enough to test its value. Thus, though a number of substances was used, the time taken up by their use, and the consequent interruption to the use of nitrite of sodium—which was his sheet-anchor, so to speak—was not great. Moreover, the patient was quite anxious to assist me, and seemed quite pleased to find in himself such a well-used field of investigation; the more so, as my main object, unless in one or two instances, was to find for him a remedy which would have a more persistent action than the nitrite of sodium.

Briefly stated, the conclusions to be drawn from the present experiments, and from those with nitrite of sodium, nitroglycerine, and nitrite of amyl, reported in my previous paper,[1] are that nitrous acid in any combination, whether as an ether or a metallic salt, is useful in the treatment of angina pectoris; and, that, in the case of the nitrite of amyl, the action of the acid is aided by that of the base. On the other hand, all compounds of nitric acid, whether ethereal or metallic, are without effect, unless it so happen that the constitution of the nitrate is such that it decomposes in the body with the liberation of

[1] *Loc. cit.*

nitrous acid. Further, nitro-substitution compounds have likewise no remedial effect.

So far as at present known, the nitrogen-containing remedies for angina pectoris may be divided into two classes, the one consisting of combinations of nitrous acid with metallic oxides or alcoholic radicals, the other comprising a peculiar class of nitric ethers, obtained from the higher alcohols, whose decomposition within the body results in the production of nitrous acid. In both classes the action of the compound is ultimately dependent on the nitrous acid present. Typical examples of the first class are nitrite of sodium and nitrite of ethyl, and, of the second class, nitro-glycerine. To these classes might be added another containing such substances as compounds of amyl, whose action is similar to that of nitrites. But, limited as this group at present is to compounds of amyl, it is not one to be chosen in the treatment of angina pectoris. The dose required is large, and the action is not rapidly produced, and disagreeable after-effects are apt to occur; and altogether I am very doubtful of its always acting so well as it did in the case of my patient.

ON TRANSPLANTATION OF SKIN-FLAPS FROM DISTANT PARTS WITHOUT PEDICLE.

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About eight years ago, I brought before the notice of the profession a new method of transplanting skin-flaps from distant parts without pedicle for the purpose of correcting eversion of the eyelids.[1] This operation has since been adopted by ophthalmic surgeons of different countries to a greater extent than I could have expected in so short a period, and has been favourably reported of by them. As the operation has a wider range of applicability, and may be practised for correcting deformities in other parts, I propose to give in this communication a *résumé* of our experience with regard to it.

Cicatrices caused by burns, wounds, and ulcerations have always proved difficult and perplexing to the surgeon who has to deal with them, and when these cicatrices are situated in the neighbourhood of joints or coverings of important organs, their proper treatment becomes of very serious importance.

It is now nearly 300 years since Tagliacozzi published his great work on plastic operations, and in it laid down the rule, which has ever since been considered as the fundamental law and *sine quâ non* to the success of the operation, that the flap must retain its connection with the adjacent living structures by a pedicle to be severed only after complete union and cicatrisation of the raw surfaces. This principle, as applied to the transplantation of skin, has in my experience been a source of great embarrassment, and has tended to retard rather than

[1] *British Medical Journal*, Sept. 18, 1875.

further the progress of plastic surgery. I noticed this many years ago in La Charité of Paris, in connection with the labours of the late eminent surgeon, Prof. Velpeaux ; and my subsequent observations in the transplantation of structures from the lower animals and in skin-grafting have, in my opinion, demonstrated that in most cases the pedicle is not essential to the vitality of the flap.

M. Reverdin introduced a method of skin-grafting in which little bits of the size of a pin's head are taken and arranged in mosaic fashion upon the ulcer, or upon the site of deficiency of skin. While practising that operation, I was never satisfied with the macadamised appearance of the parts. I also noticed a very important fact in connection with skin-grafting, namely, that the graft which was taken clean adhered satisfactorily, while the bits which had a bleeding under-surface did not adhere to their new site. I thus became convinced that the cause of non-success in transplantation was the areolar tissue underneath, and that, if we could transplant a skin-flap free of that subjacent tissue, we should secure its adhesion and incorporation. To put this to the test, I operated in one case in which the skin required for the eyelid was two inches in length by one inch in breadth. I removed the flap from the forearm in three portions, separating the first from its cellular tissue as closely as compatible with the integrity of the flap, but turning up the other two after removal, and with a knife slicing off the areolar tissue so as to leave a white surface, which I then applied to the eyelid. The difference between these flaps was very remarkable. The two which were previously prepared healed by agglutination, without even desquamation of the cuticle. Twenty-four hours after the operation, the surfaces looked pale, but the next day the temperature was normal, and the appearance healthy. The part which had been applied without previous preparation looked rather livid the first day, improved for the next two days, but on the fourth began slightly to suppurate, and, after a hard struggle for life, only a portion of it remained and the rest shrank. This however, did not compromise the result of the operation, which was on the whole satisfactory, and I was therefore enabled to formulate the conclusion that, if we wish a skin-flap to adhere to a new surface by first intention or

agglutination, we must be sure that it is free of all areolar tissue, and properly fixed in its new place. When thus prepared, we may cut the flap of any shape or size from any other part, or from another person, and transplant it without pedicle.

CASE I.—My first case—that just referred to—was that of Peter C., quarrier, 25 years of age, who had had his face, eyes, and eyelids injured by an explosion of gunpowder. The upper lid, which was strongly everted, I partially succeeded in correcting by Reverdin's method of skin-grafting; the lower I corrected by this new method. I was thus able to compare the two operations, and to report the striking advantage of my flap-transplantation.

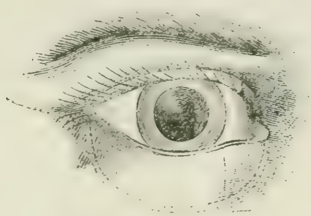

FIG. 1.

Fig. 1 represents the lower eyelid as formed with skin from the forearm. As the eyelid was completely everted, its integument totally destroyed, and the skin of the face consisted of discoloured cicatrices, not by any means suitable for plastic operations, I formed a new one in the following manner. The edges of the upper and lower eyelids having been vivified at the expense of the inner border of the lids, leaving the outer lip and the lashes untouched, I introduced three ligatures into the border of the lower eyelid, and intrusted them to my assistant. By means of these ligatures he used traction, while I dissected the whole of the cicatricial tissue, and thus liberated the lid from the adjacent structure. The ligatures were then introduced into the upper eyelid, and the edges of the upper and lower lids thus united. I then elevated the edges of the wound, preparing them to receive the new flap like a watch-glass. This patient was exhibited at the Glasgow Medico-Chirurgical Society in April, 1876, eight months after the operation, along with an

additional case; and the cases were published in the *Medical Times and Gazette* in June of the same year.

The shape and size of skin required must be carefully cut out in lint. The piece of lint is then laid on the forearm which is in a state of semi-supination, and the shape traced by the point of a knife, making it larger all round to allow for shrinking. Fig. 2 shows the size of one of the flaps which I have transplanted for the formation of the lower eyelid.

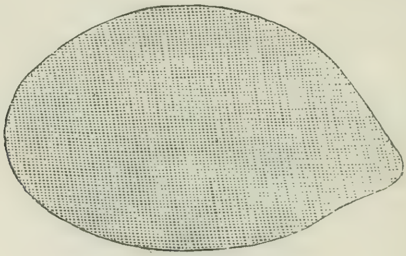

Fig. 2.

I find that the most satisfactory way of removing the subcutaneous tissue is to snip it off with sharp scissors from the flap spread out on the left fore-finger, then dip the flap in tepid water, and dry it properly.

Dressing and after-treatment.—After the flap has been prepared, it is put on its new site and moulded into position. I prefer not to use sutures for keeping it in place, as the very finest threads sometimes give rise to exudation of pus when the ligature is withdrawn. The best method, if practicable, will after all be that adopted in my first case, namely, to dissect the neighbouring part all round, and push the edges of the flap under it. After the flap has thus been properly moulded into its new site, lint soaked in hot water is held upon it for five or ten minutes, and then a few other pieces of lint wrung out of hot water are laid upon the new flap, and the whole covered and secured by an immovable bandage. The application of carbolic acid, or any other irritant substance, should be avoided as prejudicial, inasmuch as it is apt to remove the cuticle. The head should be kept steady and warm. The patient is kept in bed well covered, and supplied with warm drinks to keep up the

temperature of the body. The eye should not be disturbed for the first three days after the operation, after which the dressing should be carefully removed, the last ply of lint being properly soaked with hot water, that it may be removed easily without any dragging or derangement of the flap. It may then be dressed every twenty-four hours thereafter. I have reason to think that some cases did not succeed on account of the *nimium diligentiæ*, and teasing of the part, which requires peace and quietness for its growth. The plan of covering it only with gold-beater's skin is actually exposing it to a chill when moisture and warmth are requisite.

I would recommend also the exercise of patience in the severance of the lids. This should not be attempted for the first six weeks, and even then only done partially at first, as the too early separation of the lids is not advantageous.

The first to adopt this operation was Dr. Wadsworth, of Boston, U.S., who reported a successful case to the Ophthalmological Congress in New York, in September, 1876.

Prof. Hirschberg published another successful case of Blepharoplastik. The following year the operation was discussed at the International Congress at Amsterdam, when Dr. Martin, of Cognac, reported to the Ophthalmological section a successful case treated by my method. At the same meeting, Prof. Zehender reported three operations for the formation of new eyelids by this method. Although these did not all come up to his expectations, they may still be regarded, as I have shown in my analysis in the *Centralblatt für Practische Augenheilkunde*, as on the whole satisfactory.

In America the operation has been taken up and practised by various surgeons. Dr. Aub, of Cincinnati, reported a successful case. Dr. Reeve, of Toronto, presented two cases to the Canada Medical Association—one was a complete and another a partial success. His interesting paper was published in the *Specialist*.[1] Dr. Noyes, of New York,[2] after reporting some successful and unsuccessful cases of his own, and citing others, says :—" A number of cases have proved failures. In some of these instances failure is sufficiently accounted for; at the same time, if out of fifteen

[1] Compare reprint in *Medical Times and Gazette*, Feb. 21, 1880.

[2] *New York Medical Record*, March 27, 1880.

cases ten have proved successes, it is something remarkable when compared with those generally obtained by plastic operations." Dr. Eugene Smith, of Detroit, has favoured me with a case which he published in the *Transactions* of the American Medical Association for 1881. The photograph shows the complete success in correcting the eversion of the upper lid.

In this country, Dr. Benson, of St. Mark's Hospital, Dublin, has operated in eight cases, in which he performed my operation with Dr. Story.[1] "In five cases some of the transported flap lived, in two of these the greater portion survived, in two a smaller portion than half retained its vitality, whilst in the other one the flap seemed to slough through some of its depth, leaving the portion of it in contact with the new surface alone vital. Three were complete failures, the last being torn off, with the bandage and dressing, by the patient (a child), during the night."

Dr. Benson's paper, which he read before the Chirurgical Society, and the interesting discussion which followed, are valuable contributions; and I cannot help expressing my great satisfaction at the thoroughness with which he treated the various points of the question. In the course of his paper he says:—"Of the various plans which have been previously recommended and practised for the cure of ectropion, such as twisting of flaps, transplanting flaps from the face with pedicles, sliding of flaps, &c., all possess the serious disadvantage, from which Wolfe's operation is free, namely, that if union does not take place, and if the flap sloughs, the deformity resulting after the operation is greater than before, and the last state of that man is worse than the first; whilst, in the most successful case, the deformity of the eyelid is diminished at the expense of increased deformity of the face."

Application.—This operation is applicable to cases of deformity or loss of skin of the face, and especially of the eyelid. Its advantage over other methods is tersely put by Dr. Benson, as stated above, but is more particularly noticed in cases in which the neighbouring parts of the face and forehead are also

[1] *Medical Press and Circular*, April 26, 1882.

implicated in the burn, and so are unfit for plastic operations In such cases this operation is the only remedy.

We have lately applied this method to a case of epithelioma involving the inner canthus as well as part of the upper and lower lid. The case was published by Dr. Thomson.[1] The patient A. M'D., age 65, had, besides the epithelioma, the integument of his forehead and cheek covered with warts and melanotic nodules, and therefore unsuitable for flap-formation. Owing to this touch-me-not of the neighbouring skin, I did not even perform staphylorrhaphia, but, after removing the epithelioma, simply transferred a skin-flap from the forearm, of the size and shape of Fig. 3, and covered the gap. The results were very

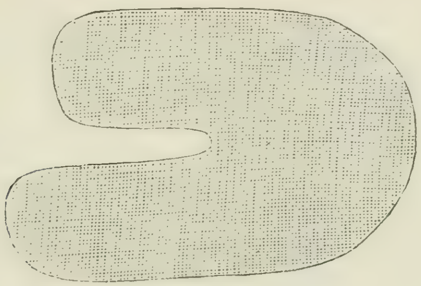

Fig. 3.

satisfactory. The case may be considered as a surgical and therapeutic success.

A word or two regarding *claims of priority*:—

Every new operation proposed has, if successful, two stages or periods to pass through. It is at first pooh-poohed, and spoken of as one that cannot be expected to succeed; afterwards, when its success has been established, some one comes forward to say that he, so many years before, had done the same thing, or something very like it. The operation described in this paper suffered considerably in its first stage. There were voices from influential quarters raised against it, but its success very speedily became established. It has now reached the second stage, and claims of priority have been advanced in favour of Mr. George Lawson, and M. Le Fort of Paris. With regard to the first, it is

[1] *Medical Press and Circular*, August 2, 1882.

stated that in 1870 Mr. Lawson corrected an everted eyelid by putting two bits of skin from the arm, one of the size of a threepenny piece, and the other of a fourpenny piece; these were not prepared as I have described, but simply cut out with a pair of scissors, and put upon the raw surface to take their chance. That such small grafts may sometimes live, if they happen to be free of subcutaneous tissue, there can be no doubt, but it is equally certain that such a procedure cannot be recommended for general adoption. This is proved by the fact that in his manual on diseases of the eye, published four years later, Mr. Lawson recommends the borrowing of skin from the neighbouring parts, but never mentions the transference of skin-flaps without pedicles.

With regard to the second claimant, M. Charles Monod has published a pamphlet, in which he states that Prof. Le Fort, after failing completely in one effort in 1870, performed a similar operation successfully in 1872, three years before my operation was published. But he adds the remarkable fact that, notwithstanding Prof. Le Fort's operation, until 1875 only five attempts had been made in that direction, whilst, "après l'apparition du travail de Wolfe, les faits se multiplièrent d'année en année avec une rapidité surprenante." That the successful operation of that eminent surgeon should have been left unnoticed in a field where the materials abound, whilst my operation was at once taken up and adopted in France and elsewhere, speaks for itself. The fact is that from time immemorial attempts have been made in this direction. These attempts have sometimes succeeded by chance, but failed in subsequent trials until I demonstrated the principles upon which the operation should be conducted to insure a successful issue. The case published [1] by Dr. Taylor will serve as a good illustration; with a little care it is quite easy to remove a flap of skin from the eyelid, without bleeding of the under surface, and this tissue is well suited for the purpose. His success in such a case is quite explicable on my principles.

[1] *Practitioner*, December, 1882.

OBSERVATIONS ON THE RELATIVE EFFECTS OF CERTAIN MEMBERS OF THE ETHYLIC ALCOHOL SERIES ON THE VENTRICLE OF THE FROG'S HEART.

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THE object in view in the following experiments was to determine the relative physiological activity of certain members of the above series. The ventricle of the frog's heart was chosen as the test tissue. The dose of any particular alcohol requisite to abolish the functions of this tissue gave the measure of the activity of this alcohol. The following alcohols were taken: methylic, ethylic, propylic, butylic, amylic—the normal alcohols of the first three were selected; of the last two the isomerics were taken, viz. iso- and pseudo-butyl alcohol and iso-amyl alcohol. In addition to these, iso-propylic alcohol was experimented with.[1] The first three members alone of the series are consequently more strictly comparable if the object in view be the relation between molecular constitution and physiological activity. But from a practical point of view the whole series must be brought in, for, of the above, propylic, butylic, and amylic alcohols constitute for the most part the fusel oil which contaminates beer, wine, and spirits. Fusel oil is a by-product of fermentation,

[1] The alcohols, with the exception of the methylic and ethylic, were obtained from C. A. F. Kahlbaum of Berlin. Normal butyl alcohol, being obtainable with some difficulty, was not taken; the iso-amyl alcohol examined was the amylic alcohol of fermentation, and commonly goes under the name of amylic alcohol; it is, however, one of the iso-primary alcohols, or a mixture of two such.

and, owing to the higher boiling-points of its constituents, alcohols, it contaminates principally the last products of distillation; amylic alcohol is its chief constituent. Rapid fermentation at comparatively high temperatures favours the production of fousel oil.

It is on the grounds of the presence of these higher alcohols, as impurities of ordinary alcoholic drinks, and of the widespread use of the latter, both as articles of diet and as medicine, that the determination of the relative physiological activity of these impurities has a distinctly practical bearing.

The literature on the subject of the influence of alcohol (ethyl-alcohol) on the circulation is, from a physiological point of view, very incomplete. The position alcohol occupies is that of a narcotic, and it is probable that its action is very similar to that of ether. The effects of moderate doses in augmenting the force of the heart's contractions and increasing the pulse frequency are well established. The sphygmographic experiments of Parkes and Wollowicz on man showed clearly the accelerating effect, but they gave no distinct indication of increased arterial pressure.[1] Nothnagel and Rossbach[2] state that after toxic doses of alcohol there may be a fall in the pulse-rate of one twentieth and in the blood pressure of one-sixth. They state that this is probably in part a reflex effect, in part a direct effect on the cardiac apparatus and vagus centre. Section of the vagi during such, according to the same authors, causes a rise in the blood-pressure and pulse-rate; when the heart action is reduced to the minimum all the peripheral vessels are found widely dilated. The above are probably the results of Dr. Zimmerberg's experiments which we find quoted in Wood (*op. cit.*). These experiments of Zimmerberg were with very large doses. According to Dogiel[3] arterial pressure is at first increased, then lowered, whilst the pulse rate shows three stages: first of increase, then of slowing, and finally again of increase.

We may take it, then, that experimental evidence is in accord with clinical experience and indicates a primary stage of stimulation which gives way to one of paralysis as the dose increases.

[1] Wood's *Therapeutics*, 1881, art. "Cardiac Stimulants," p. 119.

[2] *Handbuch der Arzneimittellehre*, 1880.

[3] Wood, *loc. cit.*

With respect to the other members of the alcohol series, Nothnagel and Rossbach (*op. cit.* p 340) state that the first five members, viz. from the methylic to the amylic inclusive, have been examined, and that their action is the same in quality as that of ethylic alcohol, but that in degree they are poisonous in ascending order, methylic being the least poisonous, amylic the most poisonous. They further give a definite numerical ratio, and state amylic alcohol to be thirty times as strong as the methylic, fifteen times as strong as the ethylic.[1] But beyond this rather bare statement there is nothing, and, in the absence of the original paper, the statement in itself is both wide and vague. There are in all four butylic alcohols, each having the empirical formula $C_4H_{10}O$; of these, two are primary alcohols, one is secondary, and one is tertiary. Similarly, the empirical formula, $C_5H_{12}O$, of the amylic alcohol is capable of eight possible arrangements ; of these, six are actually known, viz. two primary alcohols, three secondary, and one tertiary.[2] The indefiniteness of the term amyl or butyl alcohol is thus apparent. In the absence of positive statement, however, it is more than probable that the amylic alcohol employed by Gros was the ordinary amylic alcohol of fermentation, which Pasteur has shown to be a mixture of two isomeric alcohols,[3] both of which are primary alcohols. Similarly, the butylic alcohol, if not the normal, would almost certainly be the fermentation alcohol, viz. isobutyl alcohol, and the consideration would again be restricted to a primary alcohol. But the differences chemically between two primary representatives of a particular alcohol, *e.g.* amylic, are much less than between a primary and a secondary, or between a primary and a tertiary, or between a secondary and a tertiary ; and so in the absence of the normal alcohol this latter is most nearly represented chemically by its primary isomeric. Hence, we may take it that Gros's statement refers to the normal methylic, ethylic, propylic alcohols, and to the primary butylic and amylic alcohols if indeed it does not

[1] This is given on the authority of "Gros" without any further reference. In the Index Catalogue a thesis by A. F. A. Gros, "*L'action de l'Alcool amylique sur l'Organisme*," Strassbourg, 1863, is recorded. The paper itself we have not been able to get at.

[2] See Fowne's *Chemistry*, ed. by Watts, 12th ed. 1877.

[3] *Op. cit.* p. 149.

refer to the normal alcohols of these latter two; this, however, is unlikely. We may state, with reference to the present series of experiments, that they were completed before we had come across the statement of Gros, hence there was no preconceived notion as to a particular physiological relation.

The narcotic effects of the higher alcohols are stated to last much longer than those of the lower,[1] and it is also stated that the effects of alcoholic drinks with much fusel oil are characterized by their much stronger action in degree, not by a difference in kind. Results very different to these were obtained by T. Schlossberger[2] from experiments on cats, dogs, and rabbits; these were in part undertaken with Professor Griesinger. In the original paper it is stated that chemically-pure methyl and amyl alcohols were employed, but no further details on this point. The action of both is stated as quite similar to that of ethyl alcohol, but that with regard to energy absolute ethyl and methyl alcohols are scarcely surpassed by amyl alcohol. The evidence is, however, very incomplete and by no means convincing.

The present experiments were made with Roy's tonometer; the circulating fluid consisted of a solution of desiccated bullock's blood in water of about the concentration of normal blood; this was further diluted with two and a-half times its volume of saline, 0·6 per cent. The ligature was applied as nearly as possible in the auriculo-ventricular groove. The heart thus fed with the blood mixture as a rule began soon to beat spontaneously. At definite intervals of time the drug was now added to the whole mass of the circulating fluid. The dosage was maintained uniform, and was such as previous experiment had shown to be sufficient to arrest the heart within an hour approximately. The restriction of the time was for the purpose of eliminating the error due to the natural process of dying.

The functions of the ventricle were in each case completely abolished by the end of the experiment, so that the ventricle neither beat spontaneously nor responded to electric excitation. Break shocks were alone made use of for excitation.

On the subject of these alcohols two sets of experiments

[1] Richardson, quoted by Nothnagel and Rossbach, *op. cit.*

[2] *Annalen d. Chemie u. Pharmacie*, lxxiii. p. 213, 1850; also *Journal of Chem. Soc.* vol. iii. p. 180.

were undertaken. The first series were made in the summer and autumn months of 1881; the second series in November and December, 1882, and February, 1883. The first series comprised the whole range of alcohols, viz. methylic to amylic inclusive; the second included only the first three—methylic, ethylic, propylic; the experiments with these three were repeated, because, in the first series, these alcohols had not been pushed to complete abolition of the ventricular functions.

The latter series will be given first, and the list completed from the first series, the last two members of which were pushed as in the second series to complete abolition of the cardiac functions.

Methylic alcohol, CH_4O. The pure, absolute alcohol, as with each member of the series, was used. The following quantities were required:—

	I° of Room.	Quantity.
Nov. 25	17° C.	11·4 cc.
,,	18° C.	12·6 cc.
Nov. 27	15° C.	12 cc.
,,	16° C.	11·4 cc.
Nov. 29	15° C.	12 cc.
,,	15° C.	13·2 cc.
		72·6 cc. ∴ Av. = 12·1 cc. = 205·5 minims.

The heart was arrested in diastole.

As to the effect on frequency of contraction nothing special was to be noted, except towards the end stages, when, in every case, spontaneous action ceased completely, whilst the heart could still be made to contract on stimulation.

The breadth of the trace, *i.e.* the duration of the beat, became immensely broadened out in the end stages.

The effect on the latent period of the beat was doubtful. The "period of diminished excitability," or "refractory period" of Marey, was apparently slightly shortened—certainly it was not increased.

The effect of continued faradization applied to the ventricle diminished under the influence of the drug, *i.e.* the ventricle became less excitable for this mode of stimulation.

The effect of diluting the poisoned circulation with its own bulk (100 cc.) of fresh saline, 0·6 per cent. was, in one case to

cause quick recovery, but the restored beats retained their greatly-prolonged character. In a second case slight recovery appeared at the end of half revolution, some four minutes, the restored beats here were immensely prolonged. In a third case 100 cc. of fresh blood-mixture were substituted for the poisoned fluid. This should have been more effectual, but, though faint recovery occurred at the end of one revolution, the recovery was not good even after three rounds, about thirty minutes.

ETHYL ALCOHOL, C^2H_6O.

	T° of Room.	Quantity.
Nov. 15	13° C.	6·8 cc.
,,	15° C.	6·4 cc.
Nov. 16	14° C.	6·8 cc.
,,	15° C.	6·4 cc.
Nov. 17	15° C.	7·2 cc.
,,	15° C.	6·8 cc.
		40·4 cc. ∴ Av. = 6·73 cc. = 114 minims.

The heart was arrested in diastole.

As with methylic alcohol there was nothing of note as to the effect on frequency of contraction, except that here too in the end stages spontaneous action ceased completely, whilst the heart still remained excitable for electric shocks, in a lessening degree, however, the secondary coil of the Du Bois Reymond induction apparatus requiring in some instances to be pushed home.

The breadth of the beat increased in the end stages.

The latent period was not increased in the early stages; in the latter stages it was perhaps a little, but the effect was not pronounced.

The "period of diminished excitability" was shortened.

The effect of continuous faradization was lessened markedly in the end stages.

Dilution of the poisoned fluid with its own bulk (100 cc.) of saline, 0·6 per cent., gave in three cases early and good recovery. Substitution of the poisoned fluid by 100 cc. fresh blood mixture gave similarly early and good recovery. The rate of circulation through the heart was in all these cases very good.

PROPYL ALCOHOL, C^3H_8O.[1]

	T of Room.	Quantity.
Feb. 14	17°·5 C.	3·4 cc.
,,	18° C.	5·2 cc.
Feb. 15	19° C.	3·8 cc.
,,	19°·5 C.	3·6 cc.
[2] Feb. 17	18° C.	2·6 cc.
,,	18° C.	2·8 cc.

21·4 cc. ∴ Av. = 3·5 cc. = 59·32 minims.

The heart was arrested in diastole.

The end stages in each case showed absence of spontaneous action; also, in some of the cases in these stages, a markedly diminished susceptibility to respond to stimulation.

The trace broadened out in the later stages, in some cases very considerably, the diastole seemed chiefly affected.

The latent period suffered slight increase.

The "period of diminished excitability" was, if anything, slightly shortened.

Simple dilution with 100 cc. saline, 0·6 per cent., gave in four cases good and rapid recovery. The recovery set in in from two to four minutes, and rapidly increased. With the ethyl alcohol the recovery was equally or even more rapid.

In the case of all three alcohols the phenomenon of *piling up* obtained, *i.e.* at the moment of completed systole it was possible to excite the ventricle to further contraction, this latter starting from the culminating point of the preceding contraction as from a point of rest. This was well seen in the middle stages, when the height of the beat had suffered decided diminution.

The quantitative results obtained in the first series of experiments with the higher terms of the alcohol group will now be given, and the results from the complete series considered together. The numbers represent the quantities in minims.

[1] This was the normal alcohol. It was obtained from C. A. F. Kahlbaum of Berlin.

[2] The two hearts employed on this day were rather small.

Primary Butyl Alcohol, $C^5H_{10}O$. (Isobutyl Alcohol.)	Primary Amyl Alcohol. $C^4H_{12}O$. (Amyl Alcohol.)
15·3 minims.	5·95 minims.
17·0 ,,	5·95 ,,
22·1 ,,	6·8 ,,
16·8 ,,	7.8 ,,
15·6 ,,	7·2 ,,
15·5 ,,	6·6 ,,
	5·0 ,,
	8·0 ,,
102·3 ∴ Av. = 17·05 minims.	53·30 ∴ Av = 6·66 minims.

In addition experiments were made with a *secondary* and a *tertiary* alcohol, viz. :

Pseudo-Propylic Alcohol, C^3H_8O.	Pseudo-Butylic Alcohol, $C^4H_{10}O$.
48·45 minims.	40·8 minims.
39·1 ,,	32·3 ,,
45·9 ,,	30·6 ,,
28·0 ,,	39·1 ,,
49·3 ,,	51·0 ,,
56·1 ,,	49·3 ,,
266·85 ∴ Av. = 44·47 minims.	243·1 ∴ Av. = 40·5 minims.

The examination of the alcohols in the first series of experiments was more with a view to establishing a quantitative relation simply, hence they were not examined with reference to the effect on latent period, "period of diminished excitability," susceptibility to Faradic stimulation, &c., as in the case of the experiments just detailed. Nor were the dilution effects worked out. For quantitative consideration the whole series is certainly available; but qualitatively, beyond the fact that each member caused *diastolic* arrest, the consideration must be limited to the first three members of the series.

Leaving out of consideration for the moment the *secondary* and *tertiary* alcohols last mentioned, the series show the following numbers :—

205·5	114	59·3	17	6·6
Methylic.	Ethylic.	Propylic.	Butylic.	Amylic.
CH_4O	C^2H_6O	C^3H_8O	$C^4H_{10}O$	$C^5H_{12}O$

The first three members are strictly comparable, for they represent the normal alcohols. The last two, though not the normal alcohols, still are *primary* representatives, and hence, chemically, very closely approach the normal; they may therefore stand for comparison with the others. We note, then, first, the rapidly ascending ratio of activity as we pass from the lower and simpler alcohol to the higher and more complex member of the series; hence the first proposition is: *that the activity increases with the complexity.* There can be no doubt on this score; but we note further that the numbers of the first three members stand to each other very nearly indeed in geometric progression. The ethylic number is half the methylic, the propylic half the ethylic. The butylic and amylic numbers do not fit in to the series, it is true; they do not, however, show any falling off in activity but rather an increase; hence it would appear that a difference of CH_2, which is that by which each molecule in the series differs from its preceding and succeeding terms, is capable of halving or doubling, as the case may be, the activity of the molecule, or, indeed, the increase in activity may be even in excess of this ratio. The number of experiments requisite to establish definitely a precise relationship of molecular structure and physiological activity would have to be very large, in view of the considerable variations which must exist between individual frogs, even during the same season of the year. It is, however, more than probable that some such definite relation as that noted does obtain, and that each "CH_2" may be said, in a way, to have its physiological equivalent. Of course, the probability of the existence of such relationship would not be limited to this one particular series, but would apply to other instances of homologous series; and the proposition would simply be, that so long as you keep the arrangement of your units similar, so long as the lines of your structure remain the same, the addition or subtraction of each unit will carry with it a definite and constant difference. It appears that recently Mr. Perkin has demonstrated as much for certain of the alcohol series, having shown that under certain definite conditions the addition of each CH_2 corresponds to a definite quantitative difference in the degree of polarization of light which the particular alcohol is capable of effecting. This

subject is of perhaps more importance than may at first sight appear, and may hence warrant further consideration. These alcohols, confining attention to the normal representatives, present us with a series of bodies exactly similar in the *form* of their structure. This is best demonstrated by a consideration of their graphic formulæ, which would stand thus :—

Methylic ... ... $H\left(\begin{matrix}C\,O\,H.\\ H_2\quad\end{matrix}\right)$

Ethylic ... ...$\begin{matrix}H\ C\\ \ \ H_2\end{matrix}\left(\begin{matrix}C\,O\,H\\ H_2\quad\end{matrix}\right)$

Propylic ... ... $\begin{matrix}H\ C\ C\\ \ \ H_2H_2\end{matrix}\left(\begin{matrix}C\,O\,H\\ H_2\quad\end{matrix}\right)$

Butylic ... ... $\begin{matrix}H\ C\ C\ C\\ \ \ H_2H_2H_2\end{matrix}\left(\begin{matrix}C\,O\,H\\ H_2\quad\end{matrix}\right)$

Amylic ... ...$\begin{matrix}H\ C\ C\ C\ C\\ \ \ H_2H_2H_2H_2\end{matrix}\left(\begin{matrix}C\,O\,H\\ H_2\quad\end{matrix}\right)$

Each term in this series is seen to differ from its preceding term by the group "CH_2," but it will be seen that this addition does not mean derangement ; each "CH_2" thus interpolated enters into combinations precisely similar, and hence it must be apparent that whatever change in properties the addition of the group "CH_2" effects in the first instance, a like change must be effected by the addition of the group "CH_2" in the second instance, and so on. We are indeed simply dealing with a chain of like material and structure but composed of more or fewer links. There is, however, one other point for mention, viz. that such series, whilst it means *quantitative difference*, implies *qualitative likeness.*

The secondary and tertiary alcohols cannot come for comparison into such a series, for the additional "CH_2" interpolated does not now enter into precisely similar relations, but comes into relation with the group "$\underset{H_2}{C}\,O\,H$," placed in brackets above, and so modifies the general structure.

Similarly, the isoprimary alcohols do not strictly fit into the above series ; but the additional "CH_2" groups do not modify the arrangement of the group "$\underset{H_2}{C}\,O\,H$," which we are taught by chemists is the more important group in the alcohol molecule ; hence these *primary* isomerics showing much smaller differences may, without much stretch, be used for comparison.

Returning to the results of our experiments with the first three members we note that *qualitatively* there is agreement on most points. Thus, all three arrest the heart in diastole; in the later stages of toxic action the spontaneous beats are inhibited, and electric excitability is apparently somewhat lessened; the excitability for continuous faradization lessens; the "period of diminished excitability" is shortened. All three show the phenomenon of "piling up."

The primary butylic and amylic alcohols were not examined on all these various points, but they arrest the heart in diastole and they show the "piling up" just noted. We may take it, however, that the first three demonstrate sufficiently *qualitative likeness.*

The heart, we have said, is arrested in diastole; from an examination of the charts no distinct evidence of a primary stimulation in the shape of increased frequence was determinable. In the case of ethylic and propylic alcohols there did appear to be some quickening, but with the butylic and amylic members the tendency appeared to be rather the other way. There was no primary increase in the height of the trace, the amplitude diminished steadily from the beginning. This last point was tested specially in the case of ethylic alcohol,—firstly, by allowing the ventricle to become fatigued before adding the drug; and secondly, by increasing before addition the internal pressure by raising the head of pressure; in either case the ventricle at the moment of administration of the drug was emptying itself *in*completely, but in neither case did the contractions become more complete. These experiments, therefore, will not account for the clinical evidence as to the primary effect on the force of the heart's contractions, but show rather that such effect is not the result of direct action on the cardiac tissues. They are, however, quite in accordance with the enfeebled condition of the circulatory apparatus in the later stages of alcohol narcosis and show that the direct action of the drug on the tissues tends to bring about this state.

To sum up, these experiments demonstrate more definitely than experiments on the whole organism could—

1. The *qualitative similarity* of action of the different members of the alcoholic series.

2. The *general quantitative relationship*, viz. that as the complexity of the molecule increases the physiological activity increases.

3. The probability of a *further quantitative relationship*, viz. that the constant chemical difference is corresponded to by a constant physiological difference,—that each "CH_2" group increases the activity by a *definite* amount.

The third proposition obviously suggests that such relationship is to a like degree probable for other homologous series of bodies.

In addition to these results we must not forget the practical value of experiments which show, as tested by one particular tissue, a relative activity of 114 : 6·6 for ethylic and amylic alcohols, the more so that this particular amylic alcohol examined is the alcohol of fermentation and that which contaminates our alcoholic preparations. The butylic alcohol examined is likewise the fermentation and contaminating product. We may take it then as settled, that these higher alcohols are far more poisonous, though indeed the balance of opinion was already to this effect. The only remaining point to be noted is that by their direct action on the cardiac tissues these drugs are clearly paralysant, and that this appears to be the case from the outset, no stage of increased force of contraction preceding.

ON THE GASTRO-INTESTINAL FORM OF CATARRH.

BY J. H. DREW, M.R.C.S.[1]

It is in the experience of all of us that catarrhal epidemics have been prevalent during the autumn and winter, and have manifested their usual variety; probably few organs, surfaces, or tissues have escaped the invasion of catarrh—a very frequent scourge (if it may be so termed) of our comparatively temperate climate, as fever is in the tropics, and possessing equally unlimited capabilities for the production of disease. I only propose, at present, to mention three cases of the gastro-intestinal form of the disorder, variously called "stomach cold," "gastric catarrh," or "gastric fever," and by the French "muco-énterite." This malady may not, perhaps, be regarded as a very important or dangerous one, but it will be found not uncommonly to present symptoms severe enough in themselves, and interesting in their resemblance to the early stages of typhoid fever, from which, in fact, the diagnosis may be a matter of some difficulty.

Although there was no history of antecedent catarrh in these cases, each patient, otherwise in perfect health, had a distinct impression of "cold caught," or "chill taken," which was soon followed by internal pain and fever. The affection of the intestinal mucous membrane may, therefore, have been attributable to this cause, or it may have been the result of a special epidemic influence which I have had reason to believe existed during the autumn and early part of the winter.

Case I.—A young girl of eighteen was brought to me in a state of high fever; pulse, 120, temperature, 103. She was deeply flushed, very tremulous, had rigors and pains in the head and abdomen. She complained of diarrhœa, and a troublesome cough. Her tongue was thickly coated, with red tip and edges

[1] This paper was read before the Harovian Society on April 19, 1883.

and prominent papillæ. There was a disposition to sickness, but no vomiting unless she partook of solid food. Under treatment at home the attack pursued a sufficiently definite course of about fourteen days, during which the thermometer seldom registered less than 102, often 103, and the pulse continued at nearly 120, the cough became bronchial, and the bowels remained in a state of relaxation, with griping pain and tenderness, the motions being pale, semi-fluid, and occasionally containing mucus. No rash was visible at any time, nor was there any deafness or delirium. At the end of the second week the temperature, which had never varied more than a degree, fell quickly to 99, and the pulse to 85; the cough subsided, and the diarrhœa gradually ceased. By the end of the third week the appetite was restored and convalescence had commenced, solid food being then taken without difficulty, and without inducing a relapse of symptoms.

Case II.—A military man of thirty-five was taken ill after exposure on a journey, and sent for me under the idea that he was suffering from dysentery. He had a pulse of 115, and a temperature of 103, with pain in the bowels, and frequent actions, many of which were mucoid in character; and this state of things existed, in a manner very similar to the last case, for about the same length of time. The pulse and temperature varied little till after the middle of the second week, when they subsided, together with the intestinal irritation, rather suddenly, and, at the commencement of the third week, solid food was taken without causing any interruption to recovery.

Case III.—A lady of twenty-three had all the symptoms evidenced by the other two, but in a milder degree; she was not obliged to keep her bed, although she found it necessary to remain principally recumbent. She suffered from a sensation of sickness, with total loss of appetite, and pain in the bowels, and was unequal to any exertion; her pulse averaged 110, her temperature 102, and she had evacuations, more or less mucoid, three or four times in the twenty-four hours for nine or ten days. She then became rapidly better, and in the course of the third week was convalescent and able to take her meals as usual.

These cases all occurred about the same time of the year (late last autumn) and in company with many others of a less

decided nature. The debility resulting from them was very considerable, requiring the use of tonics and stimulants, and ultimately change of air; with the exception of this condition of prolonged weakness there were no further after-consequences. The treatment adopted I need not enter upon in detail. It aimed at being "rational," meeting symptoms as they arose. Probably not the least important part of it was liquid (principally milk) food, and absolute rest. The severity of the intestinal pain occasionally required the use of opium; stimulants did not seem desirable till after convalescence had commenced.

The question may arise whether these cases might not be regarded as mild forms of typhoid fever. I think not, for the following reasons.

1. The symptoms were developed promptly, not insidiously; the fever and prostration, though considerable, were not so remarkable as in an average case of typhoid, the tongue did not become brown and dry, there was no deafness or delirium, no spots appeared, and the stools were mucous in character, and unlike typhoid evacuations.

2. The duration of the symptoms and period of high temperature, which hardly reached fourteen days, were much shorter than in typhoid, and their subsidence more sudden.

3. The power of taking moderately solid food was established during the third week of the attack, and was unattended by any injurious results, such as would have been only too prone to occur in the mildest typhoid condition.

We have not yet arrived at any means of definitely diagnosing other fever states from typhoid during the early stages. If this could be accomplished it would be a great relief from anxiety for all persons concerned, although it might make no essential difference in the treatment. In the present cases there was every possible doubt as to the nature of the symptoms during the first few days, but I think it will be admitted that they soon became sufficiently distinguished from those of the graver disorder to justify my describing them under the head of "gastro-intestinal catarrh."

IS DISTILLED WATER IN ALL CASES THE BEST VEHICLE FOR EYE LOTIONS?

BY PAUL M. CHAPMAN, M.D.

THE practice of prescribing distilled water as a means of dilution for all drugs in their several applications is, perhaps, in many cases, the most reasonable and proper that can be adopted. It would, nevertheless, in some cases be advisable to remember what is lost by so doing, and that the excellence of a vehicle is, not that it should be neutral in itself, but neutral as regards the tissue to which it is applied.

I have been somewhat forcibly reminded of this by observing the relative value of distilled and ordinary water for the solution of boracic acid as a lotion for the eyes. Perhaps of all eye-lotions none is more pleasing than this of boracic acid, owing to its non-irritating and painless effect, yet, when made of distilled instead of ordinary tap-water, the use of this lotion is attended with some discomfort and smarting; the symptoms of irritation are even more marked when there is no boracic acid in solution; anybody can verify for himself that a drop of distilled water in the eye seems to be a most unpleasant and foreign element.

A moment's reflection explains this, namely, that tap-water contains salts in solution which make it slightly alkaline, thus rendering it somewhat more neutral to the conjunctiva, a tissue ordinarily bathed in the lachrymal secretion, which contains about 1 per cent. of solids, chiefly chloride of sodium.

I have tried the experiment on myself and on many of my friends, and the answer is always the same, viz. that the introduction of distilled water into the eye is attended with much discomfort and smarting, while with normal saline there is no noticeable effect whatever.

The practicable deduction is this, which I have also verified, that the addition of $2\frac{1}{2}$ grains of chloride of sodium to the ounce of distilled water renders any lotion intended to be of a soothing character much more beneficial.

ON THE PHYSIOLOGICAL ACTION OF PAPAIIN, WITH REFERENCE TO THE OCCURRENCE OF MICRO-ORGANISMS IN THE BLOOD OF LIVING ANIMALS.

BY G. F. DOWDESWELL, M.A., F.L.S., F.C.S., &c.

THE question whether parasitical micro-organisms, or their germs, exist normally in the blood and tissues of healthy living animals, is one on which in great measure depends the determination of their relation to disease; but although this has been the subject of many experiments, it cannot yet be regarded as definitively settled.

On this subject Professor Rossbach, of Würzburg, has recently published an account[1] of some observations, in which he states that by injecting small quantities of a solution of Papayotin (Papaiin) into the veins of a rabbit he found that blood taken from the heart immediately upon death, even when that occurred within fifty minutes after injection, invariably contained in each drop large numbers of active micrococci and bacteria; from which he concludes that in this case the action of a small portion of an unformed and "germ-free" chemical poison injected into the blood of a healthy animal, so altered the condition of its tissues—the occurrence of infection being here excluded—that the proto-organisms therein present normally were enabled to multiply even more rapidly than in the case of actual infection, where, he remarks, the chemical poison or ferment which is present, together with the organisms introduced, is not inoperative.

The organisms here found are unfortunately merely mentioned as active bacteria and micrococci; no description of their specific characters being given to determine whether they were

[1] *Ctbl. f. d. Med. Wiss.* 1882, No. 5, p. 82.

septic, *i.e.* putrefactive, or any known form of pathogenic species. The circumstance stated of their rapid development within fifty minutes of injection is remarkable, and if confirmed, must modify our views of the properties of these organisms, which, it has hitherto been considered, on the authority of v. Nägeli, are only capable, under favourable circumstances, of affording three generations within the hour. The properties of papaiin, an extractive from the juice of the papau tree (*Carica papaya*), have attracted attention from the remarkable circumstance of its being an active proteolytic ferment of vegetable origin, and have been investigated by several.[1] I had myself found in previous experiments that its solutions of various degrees of concentration, when incubated or kept in a warm place, in about forty-eight hours develop a species of bacillus[2] which presents some distinctive characters. It was evident therefore that Professor Rossbach's assumption (the grounds of which however he does not state) that he was operating with a germ-free solution, was mistaken. The belief that a proteolytic ferment is "antiseptic," inimical to the development of the lower fungi, is general; it is however an error, even in the case of animal pepsin, as I have shown elsewhere.[3] A solution strongly acid will not, it is true, develop or support bacteria; but if neutralised, or rendered alkaline, it will do so very readily; the acid is antiseptic and not the ferment; it is unable to dissolve the cellulose walls of these organisms, and thus may be used as a micro-chemical test for their presence in the tissues.

To determine the question here raised by Professor Rossbach's statements, I proceeded in the following manner: Of four samples of papaiin which I had received, the two best were selected; these were both of German make and very similar in their properties; both pretty active proteolytes, though I have not found any samples yet tried at all comparable in energy to what has been stated by others. Those employed were soluble in a small bulk of water, without residue, forming a light-brown or

[1] Peckholt, *Zeitsch. f. Allg. Oester. Apotheker Vereine*, vol. xvii. (1879), pp. 361-373; A. Wurtz, *Jour. de Pharm. et de Chemie*, January, 1881, and *C. R. Acad. Sc. Paris*, vol. 89, p. 495, and vol. 90, p. 1379; Brunton and Wyatt, *Practitioner*, &c.

[2] This was remarked too by M. Wurtz (*loc. cit.* p. 1379).

[3] *Practitioner*, vol. xxiv. 1880, pp. 192-201.

greenish solution, somewhat viscous and turbid in appearance, but which, examined immediately under the microscope, revealed only a very small quantity of particulate matter—mere débris—and a few highly refracting spherical or circular bodies, motionless, but to all appearance similar to the spores of bacteria, together with a few filaments resembling the degenerated cells of shrunken bacilli; they were not submitted to micro-chemical tests, as in this case the method of cultivation was more conclusive. Ten per cent. solutions were made, with all precautions against contaminations; portions of these, contained in test tubes closed with sterilised cotton-wool, were placed in the incubator at a temperature of 38 C; other tubes containing sterilised blood serum and bouillon, tested by incubation, were also inoculated through cotton-wool [1] with small portions of the two solutions; in forty-eight hours both the solutions of papaiin were filled with numbers of a form of Bacillus [2] superficially somewhat similar to one form of hay-bacillus, but rather larger, *i.e.* broader. Some cells were in active movement, others forming long, convoluted filaments with numerous spores; subsequently a tenacious whitish pellicle developed, of somewhat different appearance in the two samples, but consisting of the mycelium of the bacilli and their spores, without, however, any intersticial substance, or glia. The preparations of blood serum too and of bouillon, inoculated with the solution of papaiin, all contained numerous Bacilli similar in character to those developed in the solutions of papaiin itself.

It was thus shown that a solution of papaiin is not "germ-free," but that it readily develops at least one form of microphyte. It is, however, contrary to my observation in all previous

[1] This method of inoculating and transferring cultivations of proto-organisms was, I believe, originated, and was shown to me, by Dr. E. Klein, who has described it elsewhere; it is the only reliable method with which I am acquainted; with moderate care I have found it absolutely infallible in very numerous experiments.

[2] This Bacillus has microscopical characters which clearly distinguish it from all other species with which I am acquainted. In dried preparations it breaks up into very short segments, in length often not above twice the breadth, similar in this respect to the behaviour of the cells of B. anthracis, especially in artificial cultivations, but instead of having distinctly rectangular ends, as is the case with the latter species, they are here clearly rounded, almost acuminate, and readily recognised by this feature.

experiments, that a common saprophyte—septic or putrefactive schizophyte—should germinate in the blood of a living animal. I had invariably found that infusions of hay, teeming with their particular organisms—one of which is similar in appearance to that here in question,—when injected in such comparatively large quantities as 1 to 2 ccm. into the veins of an animal, never resulted in the germination of the microphytes in the blood, nor when the infusion used was recent—not more than about two or three days old—did any material, or, in most cases, any appreciable symptoms of intoxication follow; but when putrefaction has further advanced, the fluid became toxical through the action of the chemical products of decomposition, though still the bacteria contained in it were not able to multiply in the blood of an animal during life. Hence the question arose whether the organism which occurs in the solution of papaiin is merely an innocuous saprophyte or any pathogenic species, which, as in the case of the Bacillus anthracis, also occurs and germinates in vegetable matter.

I now proceed in the following manner: A ten per cent. solution of one of the preparations of papaiin was made and filtered; of this 1 ccm. was at once injected with a Pravaz's syringe into the jugular vein of a young, healthy rabbit, nearly full grown, previously anæsthetised; the vein was not tied, and the circulation was resumed in it after the injection. In less than an hour—fifty to fifty-five minutes—respiration ceased entirely, but, contrary to the results of Professor Rossbach, the heart continued to contract, becoming gradually more feeble for about three-quarters of an hour longer, when it finally ceased.

Blood had been taken at intervals from small arteries, but no organisms could be observed in it. The heart was now found to be somewhat distended and very flaccid, as were the walls of the large arteries, the blood appeared more fluid than normally, and had almost entirely lost the power of coagulation, as is the case with peptonised blood. Under the microscope, however, no material change was apparent in the red corpuscles, though the white were killed, had become circular and mostly granular; they were about as numerous as normally. No micro-organisms were found in any of the blood from the heart examined fresh, nor in any of the preparations made and stained by the usual

method. The small bodies, however, generally known as Zimmermann's or Max Schultze's corpuscles, were very numerous. I have reason to believe that a large portion of these, though not probably all, are mere disintegration products of the red corpuscles; their most usual form, which is somewhat distinctive, may be reproduced in any number by the action of certain re-agents. Further to test the presence of organisms in the blood, a capillary pipette was inserted into the cavity of the heart, immediately upon its being opened, and tubes of sterilised serum and bouillon, previously prepared, were inoculated with small but appreciable quantities of the blood, and kept in the incubator. They all remained perfectly clear and free from organisms for some days; as long as observed.

This experiment was repeated a few days afterwards on another rabbit in like manner, with a solution of the other preparation of papaiin. The results were similar in all essential respects to the former, but the action of the drug was not so marked; the animal lived more than double the time the first did, but neither in this case were any organisms found in the blood.

This result of the absence of any organisms in the blood on death does not affect the conclusions to be drawn from the experiments on the main point; it being shown that solutions of papaiin, or papayotin, do contain bacilli or their germs, which inoculated into suitable cultivating fluids readily develop, as they do also even in a solution of the ferment itself. Hence I conclude that it is not shown, as claimed by Professor Rossbach, that the micro-organisms which he found to develop in the blood of a living animal after injection of papayotin pre-existed there normally. In one case, and one only as yet, has it been shown, that by injecting, not into the blood, but into the peritoneal cavity of a living animal, a germ-free or antiseptic chemical irritant, micro-organisms are largely developed in the serous exudation of the "infective peritonitis" which is thereby induced; as described by Drs. Burdon Sanderson and Klein. *Med. Chir. Trans.* vol. lvi. p. 345, &c. See also *Proc. Roy. Soc.* vol. xxxiv. (1883), p. 457, &c.

These experiments were performed during December, 1882, in the Physiological Laboratory of the New Museums at Cambridge.

Reviews.

Books on Nursing.

1. *Lectures on Medical Nursing;* delivered in the Royal Infirmary, Glasgow. By Dr. J. W. Anderson. 12mo. pp. 224. Glasgow: Maclehose. 1883.

Dr. Anderson's lectures are intended for hospital nurses in course of training. They proceed on the excellent principle that, if you want a thing to be well done, you must explain to the doer not only the *how* but the *why.* The author is, therefore, not afraid that the nurse should know too much of what is properly the doctor's business. It is an unproportioned smattering of this knowledge which makes the ill-trained nurse presume beyond her true sphere. Hence an elementary (not superficial) knowledge of the general principles on which the commoner diseases are diagnosed and treated, and a fuller knowledge of the treatment so far as it is hygienic and dietetic, are essential to the training of a skilled nurse. Such knowledge will lead we are sure to better, because more intelligent, relations between the doctor, nurse, and patient. Lectures such as Dr. Anderson's are still needed in many otherwise well-organised hospitals.

2. *A Manual of Nursing, Medical and Surgical.* By Charles J. Cullingworth, M.D., Manchester. With 18 illustrations. 12mo. pp. 172. 3*s.* 6*d.* London: Churchill. 1883.

This Manual has grown out of a smaller and apparently successful little book published by Dr. Cullingworth some years ago. It gives much the same matter as is given in Dr. Anderson's lectures; but the form is better adapted for reference, while the style is, of course, less colloquial. The house-physician or surgeon will find in the book many a hint as useful to himself as to the nurse.

Injuries of the Spine and Spinal Cord. By H. W. Page, M.A., M.C. Cantab., F.R.C.S. London: Churchill. 1883.

This book is the work of an able and accomplished surgeon who has well-grounded and firm opinions of his own, and in no

wise lacks the courage of them. As surgeon to a large railway company he has enjoyed the fullest opportunities for observation and research in the special field of railway injuries. He writes in the hope that he may to some extent "succeed in throwing light upon much that is obscure, and in helping others to a clearer and more correct view than has hitherto been afforded of the injuries, and the consequence of the injuries, received in railway collisions."

The opening chapter is devoted to the consideration of concussion of the spinal cord; the rarity of its occurrence, "apart from injury to the frame, structure, and integrity of the spinal column itself," is brought out; and many of the cases so classed receive a much more likely explanation on the supposition of an intra-spinal hæmorrhage. Without denying the possible occurrence of a cord-concussion pure and simple, the author examines critically and ably the many cases placed upon record, and comes to the conclusion that in the very large majority there existed either undetected injury in the column itself or an extravasation of blood in or around the cord. And this position our own experience tends to verify, for in two instances in which sudden and complete paralysis followed a severe blow upon the spine, and in which there was an entire absence, save bruising, of all external indication of harm, thus rendering the diagnosis of concussion a fairly certain one, post-mortem examination revealed in both fracture of the bodies of the vertebræ without displacement, and with this the presence of much hæmorrhagic extravasation.

The position once established that concussion of the spinal cord, apart from disabling and usually grave lesion of its protecting structures, is a very rare lesion, the way is rendered more easy for the consideration of the so-called "Concussion of the Spine." In this condition the characteristics of true cord-injury are in the large majority of cases conspicuously absent. The patient frequently *walks* for some distance after the receipt of his injury, and may for many days continue to follow his employment, complaining only of muscular pains, nervous disturbance, and general *malaise*. Ultimately a train of painful and distressing symptoms make their appearance, but that these are due to no mysterious or undetermined injury of the cord, and are not the result of inflammatory irritation, the consequence of such injury, this book does a great deal to prove. Mr. Page dwells much, and with good reason, on the fact that after all the many thousands of persons who must have been the unfortunate subjects of this mysterious "concussion," its pathology, in the fortunate absence of opportunity for post-mortem examination, is quite undefined; and that, in the many hospitals now devoted to the special treatment of nervous diseases, such sufferers are

all but unknown. Did organic disease as surely follow this lesion as some would have us believe, then there would inevitably be seen in death as in life the tangible evidences it should present.

Under the heading of Common Spinal Injuries the author draws attention to the peculiar severity of the sprain to which the muscular and ligamentous textures of the spine are often subjected in railway collisions, and notes the important circumstance that from the temporary disablement and lack of support in the lumbar muscles, there may exist a pseudo-paralysis as regards both bladder and rectum.

Nervous Shock, its Cause and its Symptoms, are next treated of, and multifarious as these last are, that they are in no way due to organic change in the cord or its membranes is testified to by the fact that they are but those characteristic of all neurasthenia from whatever cause arising.

Here, as in the consideration of certain "Neuro-mimetic Disorders," which have their undoubted origin in this same nervous shock, Mr. Page not only proves himself familiar with mental and nervous disease, but deals in an original, clear, and philosophical way with some phenomena the causation of which, to at least one of his readers, was previously very obscure. The illustrative cases under these two headings are well told.

An entertaining chapter on Malingering, and some concluding remarks, written in a fair and perfectly unbiassed spirit, upon the harm which may follow long delay in the settlement of pecuniary compensation, close a valuable and important work.

An Appendix of Cases, carefully observed and followed up in their results, emphasise in a striking way the correctness and honesty of the author's teaching. Out of 234 cases coming under observation consecutively, only six gave any evidence of injury to the spinal cord or its membranes, and in only one of these was the recovery not wellnigh complete. Two *foreigners* sustained very severe injuries, and made rapid recoveries: was there aught in their presumed ignorance of the subject of compensation by litigation to account for this happy issue?

A Treatise on Diseases of the Liver. By DR. GEORGE HARLEY, F.R.S. 8vo, pp. 186. Illustrated. London: Churchill. 1883.

THIS is a very large, well-printed, well-illustrated volume. Of its contents we can only say, that if all that is merely rambling, or egotistic, or unscientific, were omitted, the volume would be much smaller. We cannot think it is likely to be of great use to any one in search of clear and sound teaching on liver-disease. It cannot add much to the literary or scientific reputation of its author.

A Text-Book of Pathological Anatomy and Pathogenesis. By ERNST ZIEGLER, Professor of Pathological Anatomy in the University of Tübingen. Translated and Edited for English Students by DONALD MACALISTER, M.A., M.B., M.R.C.P., Fellow and Medical Lecturer of St. John's College, Cambridge. Vol. I.—General Pathological Anatomy. 1882: Macmillan & Co.

MOST teachers of pathology have used Ziegler's Pathological Anatomy as a methodical guide to their course of instruction, and Dr. MacAlister has rendered an important service to the study by placing the book within the comprehension of every English student. His translation deserves the highest praise for clearness and simplicity. It would be easy to name several recent translations of German books which give the German idiom in an English dress, and transpose the words from one language into the other without transmitting the force or indeed the meaning of the original. Dr. MacAlister's translation is free from this kind of obscurity, and is an example of what a scientific translation should be. It may be added that Dr. MacAlister has been an editor as well as a translator, and that all his editorial changes are substantial improvements to the text-book.

The present volume begins with a section on malformations, more systematic than any hitherto set before the general student in England, though of course requiring the completion as to special organs which it will receive when the second volume of the work appears. The next section gives a concise account of the various anomalies in distribution of the blood and lymph; of hyperæmia, anæmia, dropsy in all its varieties, embolism and its allied phenomena, and lymphorrhagia.

The third section describes necrosis and the other retrogressive disturbances of nutrition. Dr. MacAlister, it may be mentioned, states, without adopting Ziegler's view, that amyloid degeneration begins in the connective tissue of organs, and that the cellular elements are only affected secondarily. The other hypotheses on this unsettled question are stated so that in this as on most points of importance the student has in a short space the general sum of pathological knowledge up to the present date. The chapter on cysts is brief, and requires some additions which perhaps strictly belong to the special pathological anatomy.

The fourth section contains much that is entirely new to English handbooks of pathology on the nature of the cell-growths in hypertrophy and hyperplasia, with an interesting chapter on metaplasia. This last contains an admirable drawing of a metaplasia of cartilage into areolar tissue from a case of fungous arthritis.

The fifth section, after three chapters on the stages and

forms of inflammation, ends with a clear statement of the respective characteristics and relations of the infective granulomata :—tubercle, syphilis, leprosy, lupus, glanders, actinomycosis. The definition of tubercle is given thus :—"We may define a tubercle, then, as a non-vascular cellular nodule, which does not grow beyond a certain size, and at a certain stage of its development becomes caseous." This nodular inflammation is due to a special virus, and this virus is defined, as a further paragraph points out, to be a special bacillus with well-defined characteristics. The statements of Koch and of the subsequent workers on his discovery are stated at length, and this section is fully illustrated with finely-cut engravings of microscopic sections. The sixth section is also fully illustrated, and will supply the student with clear ideas on the several forms of tumour. The seventh and last describes in their pathological aspect all the forms of parasites, a branch of the subject usually omitted from handbooks. Full details are given of all the forms of bacteria, with figures of every important variety. It would be well to add, in a subsequent edition, a drawing of the embryos of *Bilharzia hæmatobia* in situ in the sub-mucous tissue of the bladder; the text omits to state that the embryos are to be seen in numbers in microscopic sections of the kidney, and that the adult has recently been found by Zancarol in the vena cava inferior as well as in the veins which are specified.

To every important paragraph a short abstract of the bibliography of its subject is added, and in this part Dr. MacAlister has made judicious additions from English writings. He has gone on the principle of naming only such books as are likely to give a student further insight, and has wisely avoided any other extension of the list of books and papers. His bibliography is much better than Dr. Ziegler's, which in the body of the text is often defective. Wherever Ziegler touches upon the growth of our knowledge of a subject he is less valuable than where he is explaining the actual knowledge of the day, and he seldom causes the one to throw light on the other, as Virchow sometimes does. This is, however, an altogether unimportant defect, and is the only general one noticeable in the book.

The handbook is excellently illustrated and printed, and is of a very convenient size. It is to be hoped that it will be largely used by students, and Dr. MacAlister is to be congratulated on having chosen so useful a text-book to edit and translate, and on having done his work so well that the translation in every way surpasses the original.

Clinic of the Month.

Salicylate of Soda in Typhoid Fever.—Bareggi (*Gazz. degli Ospitali*, Dec. 3, 1882), having noticed in all cases of acute articular rheumatism treated by him with salicylate of soda, 'hat obstinate constipation occurred after two or three days' treatment, determined to utilise this action of the salicylate in typhoid fever with profuse diarrhœa. He found it to answer admirably; the diarrhœa ceased after two or three days, and the disease ran a favourable course. The salicylate may be given in larger doses than in rheumatism, without any bad effects on the digestive or the nervous system. (*Lond. Med. Rec.* Feb. 1883.)

The Cold-water Treatment of Puerperal Fever.—The results obtained by any known method of treatment in cases of septic poisoning, during the puerperal state, are, as a rule, very unsatisfactory indeed; still, some lives have been saved by the means, as yet not sufficiently known and appreciated by the profession, whose trial Dr. Rudolph Tauszky urges in given cases where other resources and modes of practice seem to be useless. The object of the administration of cold baths is principally the reduction of fever heat, if of great intensity and of long duration. Dr. Tauszky shows that these ought to be given without the least exertion on the part of the patient, as often as the temperature taken in the axilla indicates a rise of two or more degrees Fahrenheit. The water used for this purpose should be of about seventy to fifty-three or even less degrees Fahr., or, exceptionally, in very weak patients, of about eighty degrees; the patient to remain in the bath from fifteen to twenty minutes at a time Before administering the bath the precaution should always be observed of first cooling the head, by covering it with a cold wet cap or cloth, in order to prevent the sudden congestion of the brain which would otherwise follow. If the temperature does not rise above one hundred and one to one hundred and two degrees Fahr., the simple ablution or sponge bath is very valuable. This procedure, however, if repeated too often, rarely lowers the temperature more than two to three degrees, and when reaction sets in we find the

fever higher than before. As a rule, therefore, simple sponging with cold water produces rather an elevation than a reduction of fever heat, if not followed by more active measures. The friction bath, by means of a wet sheet dipped in cold water, from sixty-two to fifty-three degrees Fahr., well wrung out and wrapped around the patient, is a more useful method of using cold water for the purpose stated. In great depression or collapse, recourse must be had to the most powerful irritating procedures, such as the half bath with water from sixty to fifty degrees Fahr., and the cold douche. If the urgent symptoms are removed, warmer water may then be used, but if in spite of these measures the skin remains hot and dry he uses the wet-pack. (*American Journ. Med. Sciences*, Jan. 1883.)

Dry Local Treatment in Otorrhœa.—One of the greatest hindrances to cure in an ear disease accompanied by otorrhœa, whether the disease be due to inflammation in the auditory canal or middle ear, is the presence of granulations and polypoid growths. Yet one of the oldest forms of treatment of otorrhœal disease has been by copious syringing and instillation of various fluid medicines. Hence, in such treatment of this class of aural diseases, moisture has been repeatedly applied to, and kept in the ear, a naturally heated locality. Now as heat and moisture tend to promote granulations and keep up a discharge, it is very apparent that a moist treatment of otorrhœa in many instances has a tendency to keep up rather than to check the morbid discharge from the ear. On these grounds, therefore, Dr. Chas. H. Burnett holds that the syringe and all forms of drops should be omitted from the home treatment by the patient in cases of otorrhœa. The most the patient should be directed to do is to dry his ear according to its need, by running into the canal and down to the fundus a twisted pencil of absorbent cotton. The surgeon is to use the syringe only when it is absolutely necessary to remove by it the matter from the ear, and thus prepare the organ for the application of medication by his hand. This latter part of the treatment should consist in the blowing of powders into the ear. Of these, Dr. Burnett recommends one prepared by triturating equal parts of tincture of *Calendula officinalis* with boracic acid (gr. to minim), allowing evaporation, then rubbing one part of the thus calendulated boracic acid with one or two parts of pure boracic acid. Alum should not be used, on account of its tendency to produce furuncles. Comparative tables are given, which show that by the dry method of treatment the average duration of treatment may be shortened from two hundred and twelve days under the old plan, to thirty-four days by the dry method. (*American Journ. Med. Sciences*, Jan. 1883).

The Operative Treatment of Pneumothorax.—The following rules are laid down by Professor Weil for the treatment of pneumothorax occurring in phthisical subjects:—(1) Most cases of this kind offer but little encouragement for operative interference. Yet in some of even the most hopeless ones an operation may be the means of prolonging life. (2) In some cases with a relatively favourable prognosis, operative procedures may not only prolong life, but may even lead to a complete cure. (3) In the first five or six weeks after the development of pneumothorax an operation should not be undertaken unless the dyspnœa become so urgent as to threaten life. (4) If the dyspnœa become severe soon after the onset of the pneumothorax, and be not controllable by narcotics, then puncture of the chest-wall is necessary. If the dyspnœa speedily return, as it usually does, owing to non-closure of the opening into the lung, an incision must be made. (5) If several weeks later asphyxia threatens, it is due to an accumulation of fluid, which must be withdrawn by aspiration. Should this prove unsuccessful, then there is nothing to do but to make a free incision. (6) In cases with relatively favourable prognosis, it is advisable to operate, even though there be no danger of life. In such cases, however, it is better to wait from four to six weeks, as then the fistula in the lung will probably be closed and the fever will also have subsided. Various procedures must be adopted, according to the character and amount of the exudation. (7) In cases where the fluid is in excess and the air has nearly disappeared, the indication is to draw off the fluid in small quantities at a time. (8) In sero-fibrinous exudations we should draw off small quantities from time to time by simple puncture or by the aspirator. (9) If the exudation become purulent, Senator's method must be practised. (10) If a reaccumulation of pus soon take place—the conditions being otherwise favourable—incision must be practised at once. (11) If the fluid remain scanty and the air be unabsorbed at the end of five weeks or more, it would seem to be the most rational plan to let out the gas through the aspirator needle, and so bring the case under the seventh category, where the conditions for further treatment are more favourable. (12) If the case become stationary with a moderate amount of fluid and a considerable amount of gas, the withdrawal of both by the aspirator is indicated. (*Wiener med. Wochenschr.*, No. 39, 1882; *N. Y. Med. Rec.*)

Contagiousness of Phthisis.—Dr. Leudet, of Rouen, at the International Congress of Hygiene held at Geneva last September, gave an account of fifty-six families, all belonging to the better classes, of which in fifteen the husband was

tuberculous at marriage and the wife then healthy, and in forty-one the wife was alone affected. Of the first fifteen, in five the wives became affected, two having had relatives dying of consumption, and a third not becoming affected till ten years after her husband's death. Of the other series only three husbands became affected, and of these one had lost a sister from tuberculosis. Of five wives who became tuberculous after marriage, four had children, but only one lost any from tuberculosis. Of the ten wives who escaped, nine had children, and five lost one or more from this disease. He concludes as follows:—(1) Wives contract tuberculosis more readily from their husbands than husbands from their wives. (2) Wives who are not themselves affected may give birth to children who die of phthisis. (3) Marriage hastens the fatal termination of the disease. (4) Tuberculosis may often develop among different members of the same family at short intervals without hereditary predisposition. Dr. Poulet (*Le Concours Méd.* Nov. 1882) says that, out of sixty-four cases in which one of a married couple has died of tuberculosis at the age of fecundity, he has only met with two cases in which the other partner has become phthisical, and this after a lapse of time and with predispositions which should exclude all idea of transmission. (*Lond. Med. Record,* Jan. 1883.)

New Hypodermic Remedy for Syphilis.—Professor Liebreich brought forward, at a recent meeting of the Berlin Medical Society, a new drug for the treatment of syphilis by the subcutaneous method. This drug rejoices in the name of *Hydrargyrum formamidatum*, and is, therefore, merely a different form of the old cure for syphilis. The mode of its preparation was not stated; chemically, it belongs to the amide group, in whose structure the monovalent amidogen (NH_2) plays an important part. Liebreich was led to think of this new preparation from the notion that the ordinary amides of the body, of which urea may be regarded as the principal one, pass out of the organism in an undecomposed state; when, however, an amide is in combination with a metal, decomposition readily occurs, and the metal is reduced and deposited. Liebreich repeated his experiments before the Society, and showed that these conjectures were quite true for the metal mercury. It is supposed, therefore, that the formamide of mercury, after hypodermic injection, undergoes disintegration; and so the mercury is set free, and is able to exert its well-known power over the lesions of syphilis. The preparation is easily soluble in water, is of neutral reaction, does not coagulate albumen, is not precipitated by caustic soda, and the presence of mercury can be demonstrated by means of sulphide of potassium. The drug, when

injected under the skin, produces its effects very surely and rapidly. This is not regarded as a disadvantage, for the medicine is said to be easily borne, and has never produced salivation in Liebreich's hands. There is very little pain attendant on the injection, which has never excited any inflammation. From a half to a whole of a Pravaz syringeful (a 1 per cent. watery solution) may be injected twice or thrice daily. Liebreich looks on the preparation as the best we yet have for subcutaneous injection. (*Med. Times and Gaz.*, Jan. 6, 1883.)

Influence of Arsenic on Diabetes.—Dr. Longeville, in his thesis on the subject, states that in his experiments performed in Professor Quinquaud's laboratory he found that arsenic was a great obstacle to the formation of sugar, as shown by administering it to dogs prior to puncturing the fourth ventricle. Clinical results, as far as observed in two diabetic patients upon whom the experiment was tried, corroborated this conclusion, for a few days' dosing with Fowler's solution, progressively increased from ten to thirty drops per diem, diminished the quantity of sugar by more than one-half. The quantity of the urine passed while under the influence of the arsenic was diminished in like proportion. (*Jour. de Thérapeutique*, Dec. 10, 1882; *Med. Times.*)

Significance of Albuminuria.—Dr. A. V. Meigs, of the Pennsylvania Hospital, thus recapitulates his experience of albuminuria, based on sixty-two cases seen in private practice. (1) In no ordinary, uncomplicated case of Bright's disease should a prognosis of speedy death, or even of incurable disease, be given, for I have related cases in which the disease was chronic, lasting more than two years, and which ended in complete recovery, and others in which the person affected lived nine years. (2) Dyspnœa, usually taking the form of renal asthma, is much more common than is usually supposed, and when properly appreciated is a valuable diagnostic sign of the disease; severe coryza is a complication or accompaniment, and has also a diagnostic value. (3) Bright's disease as a cause of death is on the increase. (4) It is a very common cause of the deaths of old people, probably being the direct cause in many deaths reported as of old age. (5) The passage of gravel, even when microscopic in size, but particularly if large enough to cause nephritic colic, is a prolific cause of the disease. (6) The occurrence of tube-casts in the urine, without, or in advance of, the presence of albumen is very common; and, *vice versâ*, persons may die of Bright's disease, and the most careful examination fail to show any tube-casts, although there may be albumen constantly present in the urine. (7) The abuse of alcohol is certainly a cause of kidney disease, as proved by the

case I have related, in which it has, again and again, caused hæmorrhage from the kidney, with the temporary presence of albumen and tube-casts in the urine, disappearing again with the cessation of its consumption. (*American Practitioner*, Jan. 1883).

Oil of Turpentine in Egyptian Endemic Hæmaturia.—Dr. Wortabet gives an account of a case in which he cured a patient, aged 20, of hæmaturia. The urine when first examined contained blood, and also ova and free embryos of the *Bilharzia* in large quantities. At first the patient was treated with large doses of quinine without any change in the symptoms. Then oil of turpentine was given in doses of one drachm three times a day; this was continued daily for about three weeks, during which time it was observed that the ova became broken, and no living embryos were seen, until, at the end of the three weeks, all symptoms had disappeared and the urine was healthy. This is the only instance recorded of any drug having perfectly cured this kind of hæmaturia, so common in Egypt. (*Lancet*, Dec. 9, 1882.)

Paroxysmal Hæmoglobinuria.—According to Boas paroxysmal hæmoglobinuria is a disease *sui generis*, which must be sharply distinguished from all other kinds of hæmoglobinuria. Each attack is invariably due to cooling of some part of the skin, especially those parts which are most exposed—hands, feet, nose, or ears. The intensity of the general symptoms and the coloration of the urine are in proportion to the intensity and duration of the chill. The natural paroxysms, and those produced in experiments, are identical in their symptoms. The primary condition in paroxysmal hæmoglobinuria consists in destruction of the red blood corpuscles, and the passage of hæmoglobin into the plasma. The general symptoms are secondary. The cause of the easy destructibility of the corpuscles probably depends upon a diminution in their power of resisting the action of external irritants. The destruction of corpuscles at first occurs only locally in those places which are exposed to cold, and from thence the products of their destruction pass into the blood. The cause of their diminished resisting power is doubtful in the majority of cases. In some it is probably syphilis, and in others malaria, and the treatment, besides being prophylactic, must be directed to the removal of syphilis or malaria when present. (*Deut. Archiv für klin. Med.* p. 355, vol. xxxii.)

Extracts from British and Foreign Journals.

Treatment of Placenta Prævia.—Dr. Hofmeier's conclusions and methods claim our attention on account of the excellence of his results. His experience extended over forty-six cases, thirty-five of which were delivered in one year, and thus offers an excellent chance to judge of the method carried out by him. He first excludes from the forty-six cases three who were so far gone from hæmorrhage when he arrived that there was no chance for any treatment. Of the remaining forty-three, in nineteen the situation of the placenta was central, in sixteen lateral, and in eight marginal—a very large percentage of central placentations. The usual rule of treatment is to tampon until the cervix is sufficiently dilated. This rule the author opposes. He scarcely ever uses a tampon, and as to the cervix his rule is only to wait till clear symptoms of labour set in, *i.e.* either uterine contractions or funnel-shaped dilatation of the cervix. He then proceeds as actively and speedily as possible. This rule was followed in thirty-seven of the forty-three cases, after unfavourable experience in other methods with the rest. In nineteen cases the cervix was partially dilated, in eighteen either entirely closed or with only a funnel-shaped dilatation. The earlier the operation the more of necessity is the choice of it limited to the combined external and vaginal version with one or two fingers, the Wigand-Braxton-Hicks method. This was done in thirty cases, the foot was brought down in three breech cases, three times internal version was performed, and once the forceps applied. The combined turning was practised as long as possible, and the hand introduced into the uterus only when absolutely necessary. The feet, having been guided to the os, are seized, and by firm traction the buttocks effectually stop the hæmorrhage. In cases of central position of the placenta, the author, in spite of all the arguments against it, is in favour of perforating the placenta, and bringing the feet through. He did it in five cases, in three of which it was necessary on account of haste, and in two of which the child was already dead. It gives the mother the best chance, and

the child's chance is by any method in such a case extremely small. The rest of the delivery, the author expressly states, should be *slowly* accomplished. The condition of the child may modify this rule, but even this must not make us increase the mother's risk. "The physician must have the courage to let a doubtful child's life be lost in his hands, rather than subject the mother to increased danger. The child is to be delivered *slowly*." Even so, the author's results were not bad as regards the children. Of thirty-seven, seventeen were already dead; of the twenty still living, six died (three premature, and three from perforation of the placenta). Altogether, sixty-three per cent. died, and thirty-seven per cent. lived, which is up to the usual standard. The statistics as regards the mothers, however, are much better. The author considers in them not only the immediate result, but the after course of the case. In each case ergotin was given subcutaneously during extraction, and the uterus was washed out afterward with a five per cent. solution of carbolic acid. Of the thirty-seven patients treated by these rules, *one* died. She had been treated for twenty-four hours by tampon, and the placenta was foul and offensive when the delivery took place, and she died seventeen days after from phlegmon and phlebitis of the thigh. The author believes she would have surely been saved if action had been prompter. This one case, out of thirty-seven, gives a mortality rate of 2·7 per cent., which is far above any published rate, others having been 10 per cent., 16 per cent., and 40 per cent. After-hæmorrhages occurred in some cases, but none which could not be controlled with ergotin, ice, and hot-water injections. Of the six cases treated at an earlier date, and by the *waiting* method, one died; two had a long severe lying-in; four children were dead. Of the whole forty-six cases, therefore, five died—10·8 per cent. The author adds two useful hints as to the situation of the placenta. In nearly central situations, the smaller portion is on the lateral side, which is more loosened from the cervix lip. In placenta prævia the proportion in favour of the right side is about 11 : 4. (*Zeitschrift f. Geb. und Gynæk.* No. 8, 1882.)

Operations for Cerebral Injury.—Dr. E. C. Seguin thus concludes a note on cranio-cerebral topography. The indications for trephining after cranial injuries for the relief of symptoms of cerebral irritation, compression, or disorganisation, may be provisionally stated as follows: (1) When aphasia supervenes immediately or within a few days or weeks after an injury of the anterior portion of the head on the left side. It is extremely probable, in the first case, that a clot or bony spicule will be found compressing the speech-centre; and, in the second, that an abscess has formed either in the same part or close to it.

(2) When simple hemiplegia, or hemiplegia with hemispasm, follows an injury, however slight, in the temporo-parietal region. If the spasm or paralysis be limited to one limb or to the face, the indication to operate is even stronger. Even if the injury be not directly over the motor area, the surgeon is justified in such a case in exploring that area. (3) In conditions of coma after cranial injuries, sometimes without external wound, in which meningeal hæmorrhage is the cause of death, the discovery of slight hemiplegia should call for trephining planned according to the rules above laid down, as in Weir's case. Dr. Seguin suggests that a latent hemiplegic state might be discovered, at least in some cases, by an increase of peripheral temperature, as of the fingers or toes, on one side, and by the presence of congestion or of an erythematous blush on one buttock. (4) In the very rare cases in which the paralytic phenomena are found on the same side of the body as the cranial injury, it might be proper to trephine on the opposite side of the skull in search of hæmorrhage or fracture, the result of contre-coup. (5) In chronic epilepsy after traumatism of the head, the indication for operation is present, but it is not a specific indication, connected with the subject under consideration. A lesion of any part of the skull may be a cause of epileptic attacks, irrespective of the motor centres.

Contra-indications to trephining may be thus enumerated: (1) Whenever in apparently favourable cases there are signs indicative of lesion of the base of the brain, such as palsy of several cranial nerves, neuro-retinitis, or Cheyne-Stokes respiration. (2) When hemiplegia is accompanied by great anæsthesia, rendering it probable that the lesion is beyond the motor area, deeper and farther back. It should be understood that these indications and contra-indications are formulated from the standpoint of the neurologist. The surgeon, upon general grounds, will doubtless often modify them, and add others. It is obvious that our knowledge of cranio-cerebral topography is not perfect, and, in a practical point of view, very unsatisfactory. As yet, we have broken only a little ground; the great work remains to be accomplished. In a field so vast as this our progress must necessarily be tardy, and many hands will be required to solve its mysteries. (*Archives of Medicine*, Dec. 1882.)

Extension of the External Nasal Nerve in Cases of Ciliary Neuroses.—M. Badal considers that this method of treatment may be advantageously adopted in cases of pure neuralgia of the fifth nerve not associated with any manifest lesion of the globe. It is not appropriate to those cases in which the pain arises from keratitis, iritis, nor, of course, in glaucomatous cases. To resort to forcible extension of this branch

of the fifth to relieve pain is, he remarks, a very natural proceeding; for the ciliary nerves distributed to the globe all contain sensory filaments supplied by the ophthalmic branch of the fifth, which proceed either from the nasal or from the ophthalmic ganglion, which derives its sensory filaments from the same source. It is not surprising then that traction exerted on the external nasal branch and transmitted to the nasal trunk should modify the innervation of the branches which spring from it. The infratrochlear, or external nasal nerve, is a continuation of the nasal branch of the ophthalmic, after it has given off the internal nasal or ethmoidal branch, and issues from the orbit in contact with the periosteum between the tendo palpebrarum and the pulley of the superior oblique. It gives off nasal and palpebral branches. Badal gives the following rule for finding it. Place the index finger on the globe immediately below the superior orbital margin, the palm or surface in front, and the extremity of the finger resting on the side of the nose. The point of emergence of the nerve is found about opposite the middle of the nail. The incision to expose the nerve should be curved, following the line of the inner margin of the orbit, and extending from the tendon of the orbicularis to the supraorbital notch, where it will be easily found. Badal states that he has had cases, which he reports, in which stretching of the nerve succeeded when other means had failed, not only in relieving pain, but in modifying the pathological processes referable to the vasomotor disturbance, which in the form of secretory or trophic lesions may accompany the pain in question. Rupture of the nerve is a matter of small importance, since the anæsthesia induced is extremely limited in extent; indeed, he adds in a note that since the paper was written he has satisfied himself not only that no ill-effects follow the rupture, but that on the contrary it is better to endeavour to accomplish the rupture. (*Gaz. Hebdom. de Bordeaux*, Dec. 1882.)

Prevention of Myopia.—Weber gives the results of his examination of the eyes of the students in the higher schools of Darmstadt. He arrives at the conclusion that in view of the injurious results of poor illumination the windows, where skylights cannot be introduced, should not extend lower down than the height of the scholars when standing upright; where there are windows already he thinks they should be provided with ground glass up to this height on the south and west sides in their entire extent. Rooms for drawing and female handiwork should be lighted from above. In view of other demands connected with the question of illumination, a revision of the laws governing the building of schools is urgently advised. Then, again, in

regard to the attributes of a good school-bench, he directs the introduction of Lickroth's school-bench with a desk 50 cm. or 20 inches broad. For drawing and female handiwork they should be replaced by other apparatus. In view of the different size of the scholars of the same class seats should be provided of corresponding height and appropriateness at the beginning of each term. In order to provide for sufficient ventilation and to obviate the injurious effects of prolonged sitting, instruction should be limited to three quarters of an hour at a time and the remaining fifteen minutes should be devoted to gymnastic exercises and to drilling. The teachers should be instructed to see that the distance between eye and work should be at least 35 cm. (13 inches), and that the requisite amount of light, to be determined by appropriate trial plates, is present. Since a consideration of the materials of work is important, all printed matter not in accordance with principles that are now well established, should be discarded, especially chequered blank books and models for drawing, badly-printed maps, and too fine needlework. Weber directs that special attention should be paid to sewing, as he believes it to exert a very prejudicial influence on the eye—a complete reform in this department of education is demanded. The present style of penmanship deserves attention, and the angular forms of letter characteristic of German current hand should be replaced by a round hand. Dictation, being unprofitable, should be forbidden or only allowed for the shortest notes. Lastly, in view of the necessity of constant medical control over the hygienic necessities of the school, a member of the supreme medical board is to be provided with sufficient administrative and executive authority, or a special physician should be commissioned for that purpose. (*Archives of Ophthalmology*, vol. xi. No. 2, 1882.)

Movements of the Human Heart and Irritability of the Heart and Phrenic Nerve.—In the February number [PRACTITIONER, xxx. 145] we referred briefly to the following case. Some of the results are so significant that we give a fuller account of them now. It will be remembered that Professor Ziemssen and Dr. Gregorians utilised the opportunity presented by a patient, in whom the removal of a considerable portion of the chest-wall along with an ecchondroma left the heart covered only by the integuments, to make some interesting observations upon the heart's movements. They report as follows:—The apparatus used was Marey's cardiograph; it was simply applied by hand to the heart with the patient in a sitting position. The active movements which are most readily observed are contraction of the ventricle, and pulsation of the pulmonary artery less markedly: contraction of the left auricle, and pulsation of the

left coronary artery. This artery became much contorted when distended by the cardiac systole. By deep inspiration the diaphragm sinks an inch and a quarter to an inch and a half, and the heart follows it, sinking passively downwards to the left. During forced expiration by coughing or pressing, the heart is driven into the opening of the thoracic wall : but this seems to cause the patient no inconvenience. In deep inspiration a narrow fold about three-quarters of an inch long inside of, and nearly parallel to, the vertical coronary artery, rises up : this fold contains the left phrenic nerve. On direct palpation the ventricular contraction gives to the fingers a feeling of fulness and thickening, gradually diminishing during diastole. The pulsations of the pulmonary artery is felt as a rapid dilatation of the vessel with great tension of its walls. The closure of the semi-lunar valves is clearly felt as a strong short impulse on the surface of the artery. The dilatation of the pulmonary artery during cardiac systole is limited by deep inspirations, when the artery appears flattened, and the pulsations less. Palpation of the diaphragm clearly shows the yielding nature of the muscle, and by percussion of the diaphragm the stomach distended with air can be made to vibrate. Nothing new was learnt from auscultation of the heart. As the left lung was much contracted, no observations of any value could be made upon the apex beat. As a rule the apex moved during systole towards the right, and did not move downwards. Cardiographic tracings showed a higher curve from the left than from the right ventricle. The curves from both ventricles showed :—First, an ascending limb in which there was a break about the first third corresponding to the contraction of the auricle. Second, the summit appeared as a simple point. Curves of double and treble summits were never obtained. In the descending limb there was always one, and often two, slight breaks. These corresponded to the closure of the aortic and pulmonary valves. When two were present, the upper one corresponded to the closure of the aortic, and the lower to that of the pulmonary valves. The lower was almost always the best marked. Curves from the different parts of the ventricle show that the energy of the apex beat increases towards the apex, whilst the breaks in the descending limb of the curve, marking the closure of the semi-lunar valves, and the break in the ascending limb due to the auricular contraction, are best marked at the base. No evidence of contractions of the ventricle in parts was ever obtained. Curves from the left auricle had a tricrotic and dicrotic descending limb. The curve from the pulmonary artery is polycatacrotic, but the valvular jerk is best marked.

Professor Ziemssen utilised his opportunities for investigating the action of various influences upon the circulation, and tested first the effect of pressure upon the heart and pulmonary artery.

Very slight pressure on the ventricle with the finger caused it to give double beats; the second pulsation in each double beat being rather shorter than the first. During the pressure the energy and duration of the ventricular action increases, and the heart gives to the hand a feeling of a somewhat convulsive stronger and slower action. This double contraction occurs simultaneously in both ventricles, and is never limited to the ventricle which is irritated. This change in the ventricular action is also readily recognised in the tracing from the pulmonary artery. When the pressure is very great, a complete *delirium cordis* occurs, and every trace of regularity is absent from the sphygmographic curve in which the tracings of each pulsation are also very small. Pressure on the pulmonary artery gives no constant result, sometimes it increased the rapidity, and at other times slowed the pulse. It always rendered the right ventricle irregular, and its contractions smaller. Pressure on the distal end of the pulmonary artery readily altered the form of the curve which it gave. It was very extraordinary what very slight discomfort the patient perceived when pressure was made on the heart and on the pulmonary artery. Compression of both femoral and subclavian arteries caused only a moderate slowing of the pulse; but the effect here was complicated by mental excitement in the patient, when irritation of the phrenic nerve by a faradic current caused tetanus of the diaphragm, which lasted so long as the current was maintained. When the irritations were rapidly repeated, the diaphragm reacted to them with the same promptitude and energy as do other striated muscles.

A kind of struggle between the action of the electric irritation and that of the normal nervous stimulus was only observed when very weak currents were used. No pain was felt during the tetanus, and especially no pain was felt in the left shoulder, so that Leuchka's idea that the fibres passing to the pleura, pericardium and peritoneum, from the phrenic nerve are sensitive, is very doubtful. To constant currents the phrenic nerve reacted exactly like other motor nerves.

On irritating the heart directly by electrical currents, it was found that strong constant currents altered the action of the heart greatly, whereas interrupted currents had only a slight action. The faradic currents even when exceedingly strong did not alter the rhythm, but when continued for some time rendered the ventricular diastole less complete; they caused no sensations in the heart. The constant current, unlike the faradic, acts very considerably on the heart, so that the energy and form of contraction, as well as the frequency and rhythm of the pulse, can be regulated by it. When a feeble current is used, the alteration in the normal rhythm is inconstant and temporary. There is here a kind of struggle between the stimuli of the normal rhythmical

nervous innervation and the galvanic current, so that sometimes the one and sometimes the other has the advantage. When the strength of the current is increased this struggle disappears and gives place to a constant electrical rhythm, in which every single electrical stimulus is followed by a contraction of both ventricles, which can be both seen and felt. The more powerful physiological stimulant action of closure at the negative pole is seen in the heart as it is in ordinary muscles. The normal rhythm of the heart with eighty pulsations in a minute could be readily changed into an electrical one of 140 in a minute. With this rapid pulse the height of each pulsation was under the normal. The normal rhythm could also be influenced by the continued passage of a constant current through certain parts of the heart. These parts reach from the auriculo-ventricular groove for two centimetres down over the ventricle, and on the left ventricle behind the vertical branch of the coronary artery, and on the right immediately in front of, and upon the phrenic nerve. When the current is passed through these points, the pulse is increased to twice or three times its normal rate. Besides this the excitability of the cardiac ganglia is so greatly increased by the passage of the current, that after a little a current of $\frac{1}{6}$th or even $\frac{1}{8}$th the strength is sufficient to produce the phenomenon, and then stimulation of the left auricle, or even of the apex of the left ventricle, will also produce the acceleration. It was also noticed that the stimulus which produced any effect at all, produced as much effect as the strongest possible one, that is to say the minimal stimulation was at the same time the maximal one, just as Bowditch and Kronecker have found for the frog's heart.

Von Basch found that a much stronger current was required to keep up a rhythm slower than normal, than one which was quicker than normal; and the same thing occurs in the human heart. Powerful galvanic stimulation by alternate currents can reduce the frequency of the cardiac pulsations to a certain degree below the normal, but the rhythm cannot be kept regular even by using the strongest of currents. The curves which Von Ziemssen gives exhibit a quicker pulsation coming in every now and again between the slow ones, and remind one very much of the effects occasionally observed in poisoning by digitalis.

By observations on other patients the author found that when one pole was placed upon the sternum and the other on the spine opposite the heart, it was possible by using strong alternate currents to produce the same alterations in the rhythm, and energy of the pulsations, as occurred when the current was applied directly to the heart. (*Deut. Arch. für klin. Med.*, p. 277, vol. xxx.)

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*** Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams and Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

Department of Public Health.

CHANNELS FOR CONVEYING INFECTION TO HOUSEHOLDS: HOW THEY MAY BE RECOGNISED AND DEALT WITH.

BY R. THORNE THORNE, M.B. LOND., F.R.C.P.

(A Lecture delivered at Cheltenham on March 15th, 1883.)

In selecting a subject to bring before you this evening, I felt that I should not be trespassing beyond the lines indicated by the committee who have organised this series of lectures on questions affecting public health, if I addressed my remarks to some points connected with infectious diseases, with special reference to those specific fevers, the prevention of which must be regarded as coming within the scope of sanitary administration. The importance of such a subject hardly needs to be impressed upon an audience such as that which is before me, but I may perhaps indicate the magnitude of the interests involved by quoting a few figures from the reports of the Registrar-General of England. Limiting myself to those diseases, the spread of which is admittedly to be controlled by the adoption either of efficient sanitary works, or of such sanitary measures as isolation and disinfection, and excluding diseases such as measles and whooping-cough, in the prevention of which, by isolation or otherwise, special difficulties arise, I find that during 1871-80, the following deaths were registered in England and Wales:—from typhus fever, 13,975; from enteric or typhoid fever, 78,421; from simple continued fever, which, when fatal, is probably nothing less than an ill-defined form of enteric fever, 25,643; from diphtheria, 29,425; and from scarlet fever, other-

wise called scarlatina, 174,232. These deaths are essentially due to diseases which may be called preventable, and they amount in all to 321,696, in the ten years.

Total Deaths Registered in England and Wales from the Under-named Causes in the Ten Years, 1871-80.

Date.	Typhus fever.	Enteric or Typhoid fever.	Simple Continued fever.	Diphtheria.	Scarlet fever.
1871	2754	8461	4575	2525	18,567
1872	1864	8741	3415	2152	11,922
1873	1638	8793	3122	2531	13,144
1874	1762	8861	3112	3560	24,922
1875	1499	8914	2651	3415	20,469
1876	1192	7550	2004	3151	16,893
1877	1150	6879	1958	2731	14,456
1878	964	7652	1801	3498	18,842
1879	579	5860	1494	3053	17,613
1880	573	6710	1511	2809	17,404
Totals 1871-80	13,975	78,421	25,643	29,425	174,232

But the influence of these diseases upon the population cannot be judged of by the death-roll alone. For every such fatal case, there have probably occurred at least ten non-fatal attacks, and thus we come to be confronted with an additional 3,216,960 cases, or a total of 3,538,656 attacks from the preventable specific fevers. Mr. Simon, C.B., F.R.S., formerly Medical Officer of the Privy Council and the Local Government Board, in dealing with such death-returns has, in forcible language, explained his inability to give any exact statement of the total influence which such preventable diseases exert against the efficiency and happiness of our population; but in the face of this gigantic waste of life and of the capacity, whether temporary or permanent, to enjoy life which such diseases occasion, he has said:—"Of the incalculable amount of physical suffering and disablement which they occasion, and of the sorrows and anxieties, the often permanent darkening of life, the straitened means of subsistence, the very frequent destitution and pauperism, which

attend or follow such suffering, death-statistics, to which alone I can refer, testify only in sample or by suggestion." [1]

In wording the title of my lecture, I have endeavoured to indicate my intention to deal with certain of the means by which infection is likely to be conveyed to households, rather than the manner in which it is spread from person to person as the result of direct communication. But even with this limitation, the subject is far too wide to be dealt with in a single lecture, and I have thought it best to select for consideration three or four of what I feel to be among the more important channels through which households may become infected, and to deal with these in detail, rather than to attempt any general reference to all the known media for such infection.

I. In a report on an epidemic of enteric fever at Croydon in 1875, Dr. Buchanan, F.R.S., my immediate chief in the department of government in which I have the honour to serve, makes use of the following words :—"The air of the sewers is, as it were, 'laid on' to houses." [2] That significant expression "laid on" comes in aptly, as giving prominence to the special features of one of the channels for conveying infection to households, to which I propose drawing your attention.

Every one is acquainted with the method by which gas companies supply our houses with coal-gas for illuminating purposes. The gas being stored under pressure in gasometers is conveyed by means of pipes into each house, but it is there prevented from making its escape, except at the will of the occupier, by means of a well-devised and air-tight metal tap. In short, the gas is purposely "laid on" to each house. Now that which is done deliberately in the case of coal-gas, is unfortunately also often done with little, or indeed with no, less deliberation in the case of sewer-air; there being however this marked difference, that no such means exist for preventing the sewer-air from mingling with the atmosphere within the dwelling, as are available in the case of coal-gas. Let me explain more fully. From the inside of every ordinary dwelling-house there pass

[1] *Report of the Medical Officer of the Privy Council and Local Government Board.* New Series, No. II. 1874, p. 5.

[2] *Report of the Medical Officer of the Privy Council and Local Government Board.* New Series, No. VII. 1875, p. 48.

certain waste and other pipes intended to convey liquid refuse, first to house-drains without, and thence to the public sewers. It is the custom to regard these conduits as passing from house to sewer, but this evening I would ask you, in comparing them with the pipes for the supply of coal-gas, to view them rather as passing from the sewer as a centre to the periphery within our dwelling-houses. In our comparison the public sewer may be regarded as corresponding with the gasometer; the house-drain and the waste-pipes as representing the service-pipes for gas; and the so-called "trap" indoors as taking the place of the metal tap found in connection with each gas-bracket. The comparison, I admit, is not a perfect one. Thus, the conduits for coal-gas are intended to be impervious throughout their course, and the gas within them is maintained at a fairly uniform pressure; whereas nearly all sewers are provided with some means of ventilation, and the pressure of their aerial contents, though probably never equalling that within the gas-mains, varies from hour to hour with the rise and fall of their liquid contents; conditions which have doubtless largely tended to that implicit but blind trust in the efficacy of the water-"trap." We will however assume that the public sewer is provided with ample means of ventilation; that, if this be effected by means of shafts opening on the surface of the roadway, these shafts are both sufficiently numerous, and are never obstructed either by road detritus or otherwise, or on the other hand, if it be effected by tall ventilating pipes up houses and elsewhere, that due ventilation is not hindered by reason either of angles in their course, or of the friction of the ascending air within them. But even under these exceptionally favourable circumstances, I would assert that we should always regard the public sewer in the light of an elongated gasometer, the contents of which must rigidly be excluded from our dwellings. Sewer-air, even in its normal state, is a grave source of danger to health when it succeeds in entering our houses, but when the sewers receive in their course along the streets the infectious refuse discharged from houses where specific disease prevails, then their contents acquire a virulent power for mischief, and the sewer-air—harmful hitherto—is changed into an intense poison. This change for evil may be effected at any moment; it is constantly in

operation in some of our larger towns; hence all sewer-air should be regarded as potentially poisonous, and we should each one assure ourselves that our homes are free from its baneful influence. How is it usually sought to attain this end? The sole means adopted in nine cases out of ten, consists in placing at some point of the pipe which connects the interior of the house with the interior of the sewer a small body of water which is known as a "trap," and which is designed to act as a barrier to the passage of all sewer-air. The contrivance most commonly resorted to is the so-called bell-trap, an apparatus in which the rim of a bell-shaped cup is suspended in a small body of water contained within a circular depression, and in which the resistance to the passage of sewer air is measured by the extent to which the bell dips in the water. This form of trap is of all others the least efficient; it is not only one in which the waterlock constituting the trapping may at any moment be entirely removed at the will of the individual, but at its best it only provides between the house and the sewer a layer of water about one-half or three-quarters of an inch in depth, a barrier which is quite ineffectual to resist the ordinary force of sewer-air to which it is subjected from an ordinary sewer which, all but empty at one moment of the day, is at least three parts full at another. (The bell-trap and the methods by which sewer-air makes its way through it were here explained by means of a diagram.)

When either nuisance or injury to health has arisen in a household under circumstances such as I have described, some remedy is sought, and it is a matter of common experience to find that those whose aid is procured are firmly wedded to the notion that after all trapping must be trusted to, and that since one form of trap has failed another must be tried, and it thus often happens that the most efficient of all traps, namely the so-called "siphon-bend," comes to be provided in the place of the discarded bell-trap. But is the danger really thus removed? On this point I would, in the first instance, direct your attention for a moment to some experiments of Dr. Andrew Fergus of Glasgow. Dr. Fergus maintained that trapping had but little effect in keeping sewer-air out of houses, and that the resulting contamination of the air of dwellings was not so much due to occasional and tem-

porary failure in the efficacy of the trap, as to an almost constant absorption of sewer-air by the water on the sewer side of the trap, and its subsequent discharge from the house side of it. Having in view Graham's investigations as to the diffusion of gases, Dr. Fergus proceeded to make good his contention in the following way:—A glass tube was so bent as to resemble the ordinary "siphon" trap in a waste-pipe leading to the sewer, and this being charged with water, certain gases were evolved from a small vessel situated at *d* on what we may term the sewer

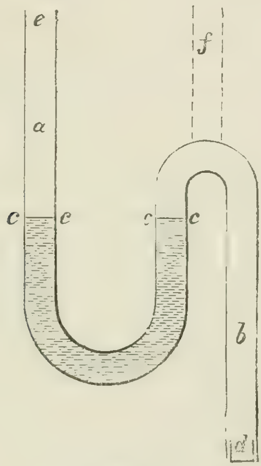

FIG 1.

side of the trap. On the house side (*a*) of the trap certain tests were applied at *e* with a view of ascertaining whether the gases had succeeded in passing through the water.[1] The results, as tabulated by Dr. Fergus, are as follows:—

Gas.	*Sp. Gr.*	*Source.*	*Test.*	*Time for reaction to show.*
Ammonia	·50	Solution	Litmus	15 minutes.
Do.	·50	do.	Nessler	30 minutes. Eat through a small wire in less than 24 hour
Sulphurous acid	2·25	do.	Litmus	hour.
Sulphuretted hydrogen	1·25	do.	Lead paper	3 to 4 hours.
Chlorine	2·50	do.	Iodide of starch paper.	4 hours.
Do.	2·50	do.	Litmus water in trap	Began to show in a few minutes. In half an hour the whole was bleached.
Carbonic acid	1·50	Generated	Lime water in trap	1½ hours.
Do.	1·50	do.	Litmus suspended over water in trap	3 hours.

[1] The diagrams, &c. are reproduced, with permission, from *The Sewage Question*, by A. Fergus, M.D., Glasgow.

These gases therefore passed up the tube, through the water, (*c-c*), and were recognised by the tests situated on the house side of the trap after the intervals specified. It was, however, urged that the results would probably be different if the trap were ventilated. A ventilating shaft was therefore inserted in the upper part of the bend on the sewer side (*f*), and the experiments were repeated. "The results," says Dr. Fergus, "were much the same, except that the reaction was a little longer in showing itself."

What, however, is the bearing of such experiments on the question at issue? Ordinary sewer-air may be taken to contain in every hundred parts about seventy-nine parts of nitrogen, nearly twenty parts of oxygen, not quite half a part of carbonic acid, and traces of sulphuretted hydrogen, marsh gas, and ammonia. These gases, however, when inhaled in the proportions indicated, can hardly be regarded as materially affecting health, and the proof of their passage through a water-trap would be comparatively unimportant were sewer-air composed of them alone. But sewer-air also contains organic matter in the form of vapour and of definite particles, and wherever sewer-air as such succeeds in making its way, there its organic constituents also go. Now, we already have proof to the effect that the contagium of certain infectious diseases is particulate; that the particles of which it consists are of an organic nature; and that the contagion, of at least some of these diseases, consists of, or at least includes, a specific living organism, able to multiply its kind. Hence it at once suggests itself to our minds that it is the organic matter in sewer-air, rather than the inorganic gases, which constitute its special danger in regard to health. It is true that doubts have been expressed as to whether these organic particles succeed, in common with the gases, in making their way through water-traps, and some carefully executed experiments of Dr. Neil Carmichael, of Glasgow, have gone far to show that they do not do so under the circumstances of a laboratory experiment in which the conditions affecting ordinary drain-traps were as far as practicable reproduced. In one experiment Dr. Carmichael, having admitted sewer-air into what I have termed the sewer side of such a trap as that experimented on by Dr. Fergus, fixed on the house side a flask containing a fluid specially designed to

facilitate the cultivation and development of organic germs, and having, in the first instance, and in addition to other precautions, raised both the fluid in the flask and the water in the siphon trap to such a temperature as to thoroughly destroy all organic life within them, he insured the passage of sewer-air through the sterilised water-trap, and over the surface of the sterilised fluid in the flask beyond. The existence of organisms in the sewer-air was again and again proved by their rapid development in the water constituting the trap, but in no instance did the cultivating fluid in the flask beyond exhibit any indication of its having become infected. In short, no organisms had succeeded in making their way through the water-trap.

Dr. Frankland, F.R.S., has also shown that it is extremely improbable that the mere flow of sewage through sewers and drains "can impregnate the circumambient air with suspended particles."[1] But in view of the fact that, in the development of gases during the processes of fermentation and putrefaction, there occurs a bursting of bubbles of gas at the surface of the fluid which causes the projection of visible liquid particles into the air to the height of several inches, he thought it not unlikely that other particles, too minute to be seen, might be simultaneously projected and continue suspended in the air for some time. Experiment proved that this was actually the case, particles invisible to the naked eye being projected and carried along by a gentle current of air for a distance of twenty-one feet. This being so, it becomes obvious that since, in the case of direct drain connections, such traps as we have been considering do, as a matter of fact, become impregnated with organic germs, it only needs that decomposition of the liquid in the trap should take place, as a consequence of the retention either of putrescing liquids or of former deposits, and then a "suspension of zymotic matters in the air" of our dwellings will be the result.

Danger too comes about in other ways. The water in traps is apt to be sucked out by siphon action, as the result, for example, of a rapid flow along the drain into which the waste-pipes discharge, and under these circumstances sewer-air and its organic ingredients pass unhindered into our houses. So also,

[1] "On the Transport of Solid and Liquid Particles in Sewer Gases." By E. Frankland, F.R.S. *Proceedings of the Royal Society*, No. 178, 1877.

traps are liable to be forced by the pressure of the sewer-air upon them. Having regard to some of Dr. Carmichael's experiments, it might at first sight be supposed that organic particles contained in bubbles of air would be detained in their passage through a water-trap, and that, so far as the production of infectious disease is concerned, the forcing of such a trap would not be attended with any appreciable risk. This, however, is by no means the case. In certain experiments carried out at the Royal Institution by Professor Tyndall, F.R.S., it was found that air passing through an experimental tube carried with it "a considerable amount of mechanical suspended matter,"[1] which was illuminated when a powerfully condensed beam of light passed through the tube, and these floating particles were subsequently ascertained to consist of organic matter. The air in question was then passed through certain liquids, but Professor Tyndall says: "The effect was substantially the same when the air was permitted to bubble through" either a body of "concentrated sulphuric acid" or a "solution of caustic potash."

I am willing to accept the accuracy of Dr. Carmichael's contention that a water-trap prevents the passage of the organic matters with which he experimented, but with a knowledge of water-traps as they exist, of the manner in which they act, and still more of the manner in which they fail to act, I have no hesitation in urging that such appliances should never be regarded as sufficing to keep out of our houses the more dangerous matters contained in sewer-air. This is indeed freely admitted by Dr. Carmichael, who, in the same paper in which he relates the facts I have adverted to, points out many dangers attendant upon such traps,[2] and he enforces the caution he gives by relating a circumstance which is referred to in a report of Dr. J. B. Russell, the able Medical Officer of Health for the city of Glasgow. Dr. Russell states that in tenements of one single apartment, having no connection with the sewer either by means of a sink-pipe or otherwise, there had been a death-rate from diphtheria of 120, and from enteric fever of 249, per million

[1] *Essays on the Floating Matter of the Air in Relation to Putrefaction and Infection*, by Professor Tyndall, F.R.S. p. 2.

[2] *The Sanitary Journal*, No. 49, vol. iv. March, 1880, Glasgow.

inhabitants. The introduction of a sink increased the diphtheria death-rate to 253—*i.e.* 110 per cent., and from enteric fever to 677—*i.e.* 171 per cent., the rate of mortality from certain allied diseases also undergoing a corresponding increase. In tenements of two apartments, the increase in the death-rate from diphtheria and from enteric fever was even greater. In the absence of knowing whether there were other circumstances calculated to have favoured this special incidence of disease upon the tenements thus connected with the sewers than the one to which it has been assigned, I should find some difficulty in asserting that the drain connection and the whole of the increase in the diseases specified were necessarily related to each other as cause and effect, but Dr. Russell's opinion that they were, carries great weight, and it is certain that Dr. Carmichael, notwithstanding the results of his own experiments as to the influence of water-traps, fully concurred in this view, for he proceeds to say: "From this we see that the introduction of a sewer connection by a sink increased materially the death-rate from the specific diseases diphtheria and enteric fever," as also from certain other affections "represented by diarrhœa and croup." In other words, the waste-pipe from the sink must be regarded as having constituted a channel for conveying infection to these households; the specific infection of certain diseases having thus been "laid on" to these dwellings.

One striking instance which further illustrates this point came under my own cognisance. Some years ago I received instructions to inquire into the cause of an outbreak of enteric fever in a small township in Yorkshire. The main incidence of the disease was upon a group of houses which formed an irregular square containing twenty-three cottages, occupied by eighty-eight persons. Up to the first week in June the inhabitants of this locality had been free from fever, but at that date a series of attacks of well-marked enteric fever occurred almost simultaneously in a number of houses, fresh attacks taking place day by day until, in the space of a few weeks, one or more inmates in fifteen out of the twenty-three cottages had been attacked, the total number of patients amounting to thirty-five. (In the annexed diagram the figures indicate the number of residents, and the black dots the number of attacks from enteric fever, in

each house.) Now it has long been recognised that when water is the vehicle by means of which the contagion of enteric fever is conveyed, a large proportion of the susceptible persons exposed to its influence are, as a rule, attacked, and they are generally attacked almost simultaneously. Bearing

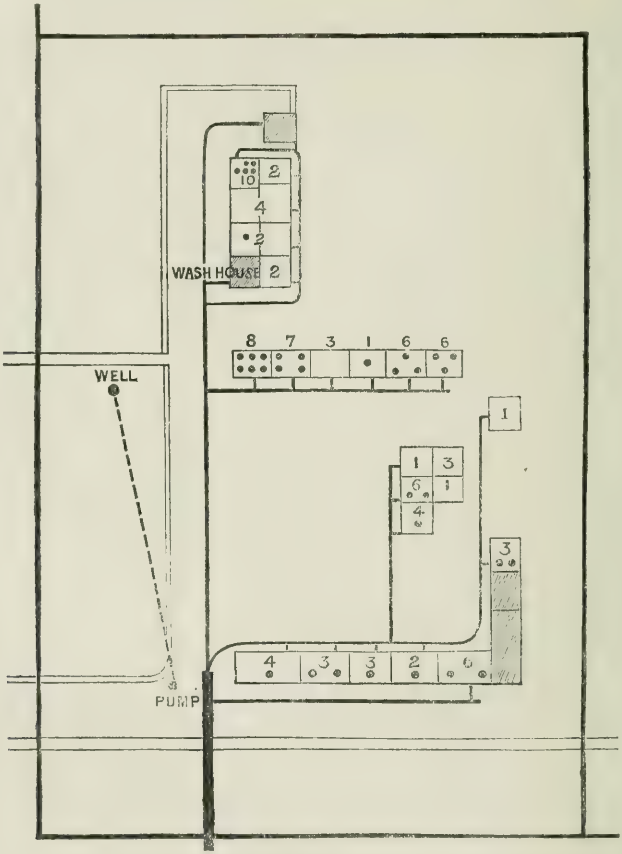

Fig. 2.

this point in view, the Medical Officer of Health for the district concerned at once suspected the water-supply for the square. This was derived from a well in an adjoining field, the water being conveyed through a pipe to a pump at the corner of the adjoining road. There is, however, in the case of

enteric fever a definite interval, generally of some ten to fourteen days, between the date of the reception of the specific poison into the system and the time of the occurrence of the first symptoms of the disease, this interval being known as its "period of incubation," and when I came to make inquiry as to the early history of this outbreak, I found that, owing to a failure in the supply of water and to certain complaints as to its quality, the owner of the property had ordered the pump to be locked, and that its contents had remained unused for a period which more than covered the period of incubation referred to. The use of this water had, therefore, no relation to the prevalence of the disease, and for some time I was quite unable to assign to the outbreak any sufficient cause.

In the course of my subsequent investigations I entered a wash-house belonging to one group of the houses in question. I was followed in by its owner, an old lady, who sought at once to satisfy my curiosity by assuring me that the building was rarely used; indeed, that the last time it was used was six weeks ago, at which date she had washed some linen there for a young man who had been very ill, and who lived some distance away with his mother, whom she had been desirous of aiding by this act of kindness. I had before this noticed that all the cottages were provided with sinks in their living rooms, and that by means of these sink-pipes, which were in unbroken communication with a drain outside, offensive effluvia at times made their way into the dwellings, these having been especially noticed towards evening, when the houses were shut up and the fires were lighted. It at once occurred to me that if the sick man referred to had suffered from enteric fever, and if the drains for the several parts of the square all communicated with the sewer by which the liquid refuse from the wash-house was conveyed away, then a specifically contaminated sewer-air had, subsequently to the last use of the wash-house, replaced the ordinary foul effluvia which forced their way into the cottages, and that in this way infection had been laid on to the several households. My surmise turned out to be correct. I found that the illness from which the young man had suffered was indeed enteric fever; and a few labourers having laid bare the drains, these were all seen to communicate with the sewer which received the

wash-house drainage; the sewer being further of such faulty construction, as to be little better than an elongated cesspool.

In view of the danger which is thus found to attach to this direct communication between the interior of a sewer and the interior of our dwellings, "What," you may fairly ask, "is the remedy?" I answer that the remedy is of the simplest kind. There is no need to imitate those who decry the use of all sewers, it is rather the abuse of them that should be avoided, and this result may be attained by breaking the direct connection which has been referred to. In the case of a waste-pipe from a sink, the pipe should be brought through the wall into the outer air and there be cut off, its contents flowing to a trapped drain-inlet outside the dwelling. (This point was explained by means of diagrams.)

This principle of disconnection is, however, of much wider application than I have as yet indicated. All waste-pipes coming from lavatories, baths, the trays beneath baths, water-closets, &c., as also the overflow-pipes from cisterns and the rain-pipes, especially such rain-pipes as have their heads anywhere near windows or beneath overhanging eaves, should, like the sink-pipes, be disconnected from the drains, an air-space intervening between them and the drain-inlets into which they empty.

There is exceptional danger in the direct connection which is often maintained between the inside of houses and the sewers by means of the overflow-pipes of cisterns. These pipes are very generally provided with a "siphon-bend," the water in which serves as a trap. But even if such traps were efficient, the water constituting the trapping is often absent; for the ball-cock of the cistern is intentionally so contrived as to prevent overflow, and hence when once evaporation of the water in the trap has taken place, sewer-air passes up it without let or hindrance. In the case of cisterns in bedrooms, the inmates of which have been struck down with enteric fever, I have several times found that the cistern-lid, once painted white inside, has presented, immediately over the mouth of the waste-pipe, a large circular black patch, the result of the action of some sulphuretted hydrogen in the sewer-air upon the lead in the paint. And when these cistern-lids are fairly air-tight, the water itself must necessarily absorb the ascending effluvia.

Adapting the same principle of disconnection to the house-drain itself, I would further urge that an air-break should always be contrived between the end of the drain and a trapped inlet leading to the public sewer; the more so, as when this is effected a further safeguard can be ensured, namely, the provision of two ventilating apertures to the drain and the maintenance of a constant current of fresh air through its entire length.

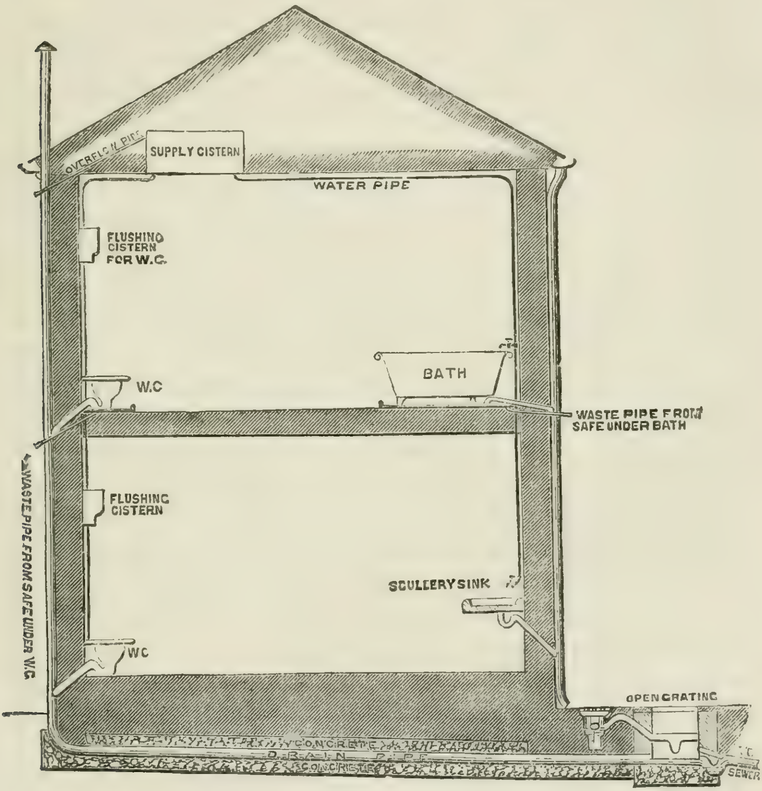

Fig 3.

[The annexed diagram, copied by permission from Messrs. Knight and Co.'s *Annotated Model Byelaws*, embodies the main features of the illustrations referred to in this portion of the lecture.]

(*To be continued.*)

REPORT OF THE MEDICAL OFFICER OF THE LOCAL GOVERNMENT BOARD, 1881.

This report appears as a supplement to the Eleventh Annual Report of the Local Government Board; it contains an account of the proceedings taken in the Medical Department during the year 1881, and it unquestionably constitutes one of the most important documents issued from that department.

In submitting the Report, Dr. Buchanan, F.R.S., the medical officer of the Board, commences by giving some account of the state of vaccination throughout the country, and of the administration of the National Vaccine Establishment, referring the reader to further details in the Appendix, which forms the main portion of the volume. He next draws attention to a report by Dr. Cory, on the preliminary proceedings of the Animal Vaccine Establishment. The report points to fairly good results, both as regards the vaccination of the calves and of the infants brought to the station. The calf-lymph is also stated to have produced fairly complete results when stored on points and used within five days of its being taken from the calf; but when stored in tubes the same degree of success was not met with. When Dr. Cory commenced his vaccinations with animal lymph, he used lymph which had been procured from the approved stock of Dr. Carsten, of the Hague, but he has since been furnished by Dr. Dubreuilh, of Bordeaux, with a new supply, which was only seventeen removes from an alleged case of spontaneous cow-pox, and he has seen reason to cultivate this latter lymph from calf to calf in supersession to the Hague stock.

The next subject referred to has reference to small-pox in London during 1881, and it is dealt with from a point of view which is both novel and of extreme importance in connection with the administration of the Vaccination Acts. In June, 1881, Dr. Buchanan submitted to the Local Government Board a memorandum on the relative mortality from small-pox in vaccinated and unvaccinated inhabitants of London, from which it

appeared that the death-rates per million of the vaccinated class were 90 at all ages, 61 under 20 years of age, and only $40\frac{1}{2}$ under 5 years of age; whereas, amongst the unvaccinated class, the rates amounted to 3,350 at all ages, 4,520 under 20 years of age, and as much as 5,950 under 5 years of age. But in this memorandum no distinction was drawn between efficient and inefficient vaccination, whereas "the power of a thorough vaccination to protect against death from small-pox is at least ten times greater than the power of much that passes under the name of vaccination." To elicit further information on this point, Dr. Stevens received instructions to make inquiry as to all deaths occurring in London during 1881 amongst persons alleged to have been vaccinated, or whose vaccination was doubtful. The total small-pox deaths in the metropolis during the year numbered 2,379, and of these 953 are reported as having occurred in children under 10 years of age, and 667 under 5 years of age. And here a striking fact is at once noted, namely, that whereas in the twenty years before 1871 when public vaccination was gratuitously provided but when compulsion was little more than nominal, the deaths from small-pox in children under 5 years of age amounted to as many as 56·9 per cent of the total metropolitan small-pox mortality, yet in the year 1881 children of that age only contributed 27·8 deaths to every hundred.

Out of the total small-pox deaths under 10 years of age there were 46 concerning which it was found impossible to procure any information as regards vaccination, but excluding these it appears that whereas out of 55,000 children under 10 years there occurred 782 small-pox deaths, there were only 125 such deaths out of 861,000 vaccinated children. In other words, "upon equal numbers of the two classes, therefore, the mortality from small-pox among the unvaccinated was about a hundred-fold the mortality from small-pox among the vaccinated." And what, it may be asked, gave this degree of protection against small-pox to the one class as opposed to the other? Obviously nothing but vaccination. But when a child is recorded in a death certificate as having been "vaccinated," that term has not always the same significance. In the first place, 8 out of the 125 children so described were found either not to have been

vaccinated at all, or to have been "unsuccessfully" vaccinated, and of the remaining 117 it was found that, whereas the number of vaccinated children in London under 10 years of age is made up in about equal numbers of those vaccinated by private practitioners and those vaccinated at the public expense, there were only 35 small-pox deaths amongst those who were publicly vaccinated as opposed to 82 amongst those who were privately vaccinated. That private vaccination should afford so much less protection against small-pox than public vaccination does, is a fact which deserves very serious attention; the more so as it is in the main due to the performance of the operation, by private practitioners, in an inefficient manner, merely because of the clamour of parents who are ignorant of the value of securing the full protection which a thorough vaccination is capable of affording.

But the actual number of small-pox deaths which can properly be regarded as having followed on vaccination admits of a much further reduction. Thus, out of the 35 children who were successfully vaccinated by public vaccinators, it was ascertained, on personal inquiry, that 12 had been vaccinated too late for them to get protection against the poison of small-pox to which they had already been exposed. We are thus left with 23 fatal cases, and of these some were found to have suffered from some disease independent of small-pox before the latter was contracted; a few had scanty and imperfect vaccination marks; and as to others no information could be obtained. There was, however, only 1 of the 23 children of whom it could confidently be asserted that it had been efficiently vaccinated and yet that it had died of small-pox uncomplicated by any other disease. But ignoring for the moment all questions as to the quality of the vaccination, what has been the result to London children under 10 years of age of submitting to the operation as performed by the aggregate of medical practitioners, as compared with that on those who remained unvaccinated? The answer may best be given in Dr. Buchanan's own words:—"It has already been stated that in 1881, among the 55,000 children who had not been vaccinated, there were 782 deaths from small-pox; that among the 861,000 children who had been vaccinated, there were 125 deaths from small-pox. If the

London children under 10 who were unvaccinated had had the protection which the current vaccination gives, not 782 of them, but at the outside *nine*, would have died of small-pox during the year.

"If the 861,000 vaccinated children had died at the rate of the 55,000 unvaccinated, we should not now be considering 125 small-pox deaths and how they can be reduced, but we should be confronted with an additional 12,000 and more deaths from small-pox, occurring during the year in the London population under 10 years of age."

It is often asserted by persons in quest of arguments against vaccination that the escape of death from small-pox amongst the vaccinated is due, not to that operation, but to the progressive improvement in sanitary circumstances, and to a consequent lessening of exposure to the influence of infection. Now, no less than one half of the children under 10 years of age in London were vaccinated at the public expense, and these children, some 430,000 in number, have unquestionably come from the poorer population, as to whom it can hardly be asserted that they are better lodged and guarded than their richer neighbours who prefer to pay for vaccination. Had these 430,000 vaccinated children suffered from a small-pox mortality at the same rate as the unvaccinated did, the number of small-pox deaths occuring amongst them would have been 6,000 instead of 35, or, more correctly, for reasons already explained, instead of 23. Thus, as Dr. Buchanan puts it, in spite "of any greater exposure to small-pox undergone by the poorer half of the community, it is seen that the poorer half had actually less mortality from small-pox among its children than the richer half; and the opinion that the escape of the vaccinated from death by small-pox was due to any smaller risk of small-pox infection encountered by the vaccinated as compared with the unvaccinated may be confidently set aside."

There was, indeed, in 1881, a saving of 12,000 lives amongst children under 10 years of age in London; lives which would otherwise have been forfeited to small-pox, and it is absolutely impossible to find anything to which this result can have been due except the operation of the Vaccination Acts of 1867 and 1871. The infantile population which formerly bore more than

half the total small-pox mortality now only bears about one quarter of it, and even this lessened proportion is shown to be almost entirely made up of those who have not had the benefit of the protection conferred by efficient vaccination. But Dr. Buchanan proceeds to ask, "Can it be asserted that this saving of life has been too dearly purchased, and that injurious effects of vaccination counterbalance its advantage?" It appears that during the year 1881, the deaths which were registered as having been associated with the performance of vaccination were 12 in number, most of them being recorded as due to "erysipelas after vaccination." In some of these cases erysipelas was ascertained to have existed in the dwelling affected before the performance of the vaccination; in others it clearly had no relation whatever to the operation; whereas in some of the remainder it is possible that careful cleansing of instruments and other necessary precautions might have prevented the occurrence of the complication. But admitting for the moment that all 12 deaths were directly due to vaccination, it results that this was the price paid for 12,000 lives that were saved. It is to be hoped that with increasing care in the performance of the operation, even this small mortality may be further diminished, and that the substantial benefit which is conferred upon the population by vaccination may not be attended with a drawback which, though trivial in one sense, must necessarily be regretted.

One consideration stands out most prominently in connection with the subject we have discussed, and it is this: The maintenance of a system of compulsory vaccination is essentially necessary in the interests of young children who, were it not for the interference of the State, would be helpless to protect themselves against a disease associated with terrible mortality, much suffering, and hideous deformity. The right of the State to interfere on behalf of women and children employed in factories and workshops, and in the matter of the education of the young, has been fully recognised on the ground that those whose interests are thus cared for are unable to protect themselves against such injury as follows on parental and other neglect. The infantile population amongst whom small-pox unmodified by vaccination always has been, and still is, so deadly a scourge, are equally helpless to protect themselves against preventable disease and

death, and in view of the proof now afforded as to the enormous protection which the Vaccination Laws have so successfully afforded to them, it is difficult to understand that any reasonable plea can be brought forward for depriving them of this benefit.

Foremost amongst the auxiliary scientific investigations which have been carried out by the Medical Department is one, the object of which has been to ascertain what value really attaches to quantitative chemical analyses of drinking waters with regard to the question of their wholesomeness or otherwise. The investigation was assigned to Dr. Cory, who, having prepared a series of samples of water by the intentional addition of various polluting matters, submitted them to certain well-known chemists for quantitative analysis. The report prepared by Dr. Cory, as also Dr. Buchanan's comments on it, deal with considerations of the highest importance, and we hope shortly to be able to refer to them in some detail. In the meantime we would merely state that in the case of waters purposely contaminated by the evacuations of enteric fever patients, no evidence indicating such contamination was forthcoming as the result of chemical analysis, and this inquiry has made it more than ever evident that however valuable such analysis of potable waters may be, it entirely fails, in some of the most marked instances of specific pollution, to afford any sufficient warning of the dangerous qualities attaching to certain contaminated waters if used for domestic purposes.

Another contribution to the volume is from Dr. Klein, who has been engaged on an investigation into the relation of pathogenic to septic bacteria. In cultivating the bacilli of disease, Dr. Klein has found changes both in form and potency, but the changes have been due, not to a true change in the original organism, but rather to the introduction of a new bacillus. Dr. Klein has further made experiments which tend to show that the virulence of the anthrax bacilli is largely due to the occurrence of spore-formation, and that this occurrence is one of the conditions favouring the potency of the organism which may come to be defined. In his cultivations of this bacillus on the lines indicated by M. Pasteur, Dr. Klein has been unable to

produce such an attenuation of the virus as would admit of its being inoculated into an animal with the certainty of producing less serious effects than follow the insertion of the original virus. Indeed, he seems to show that, although degeneration of the bacillus may follow on the exhaustion of the pabulum favouring its development, yet, whenever the resulting virus produces any effect, it is the same effect as that produced by the virus before the commencement of the process of degeneration. In referring to the result of these successive cultivations, Dr. Buchanan remarks: "There is something more than this wanted for the production of Pasteur's anthrax 'vaccin,' and the conditions of it have not transpired from M. Pasteur's laboratory." Dr. Klein's paper, which is illustrated by a series of lithographic plates, will well repay an attentive and critical perusal, and we hope shortly to recur to it.

Amongst the other papers contained in the volume are the following:—(1) A report by Dr. Greenfield, which appears as supplementary of Mr. Spear's report of 1880 on Woolsorters' Disease at Bradford. Dr. Greenfield now gives the anatomy and pathology of the complaint, and also the results of certain experimental inoculations of animals, the paper being illustrated by a series of twelve well-executed lithographic drawings of the tissues of patients and animals who had suffered from the disease. (2) A report by Mr. Victor Horseley, M.B., B.Sc., giving a summary of the knowledge available concerning the physiological relations of septic bacteria. This contribution is admittedly, by way of basis for the further study of the chemistry of septic bacteria, especially in relation to the conditions necessary for effectual disinfection, a subject concerning which trustworthy information is urgently needed. (3) Several reports of the medical inspectors of the Local Government Board, which may be regarded of more than passing interest, are reproduced either in whole or in part; and (4) Dr. Thudicum, who, it is stated, is now approaching the close of his studies into the chemistry of the brain, makes a further contribution to this subject.

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WHAT IS THE BEST TREATMENT OF ECZEMA AND OF PEMPHIGUS?

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If it be true that in the multitude of counsellors there is wisdom, it is quite as true that in the multitude of remedies there may be confusion. It seems like a frank acknowledgment that either our pathology or our therapeutics is at fault; that the one does not fit the other, so to speak, at all points; and that we ought to search for more definite science on the one hand, and more exact remedies on the other. It has been justly said that the fact of many remedies being proposed for any disease is a sign of its incurability. This may not be absolutely true, for new diseases require new observations and experiments, and so there must be frequent therapeutic inquiry and trial. But for common maladies on the outside of the body, easily studied in their etiology and progress, why should we not clearly understand what medicines to use, and how to use them? Ought there to be any difficulty in the matter, and why should there be a crowd of remedies if a few will do the work?

It is, I think, useless to try and minimise the fact that diseases of the skin have been a long time in passing through the empirical stage of therapeutics. The clinical facts are before our eyes, but we have not interpreted them just as we should interpret the plain phenomena of disease elsewhere. The scholastic ideas of a humoral pathology so governed our conceptions of the nature of skin diseases, that we missed the clue to their rational treatment. It seemed *too* easy to suppose that the skin might suffer its own inflammation, irritation, or malnutrition; there must be, or ought to be, a toxæmia at the root of it all, something which vexes and pollutes the blood, and causes a fermentation or explosion on the tegumentary surface. But wherefore should we invent theories to explain facts which without any difficulty explain themselves? What mystery is there in a catarrh of the skin and not in a catarrh of the air-tubes or of the bowels? There are the same elements of mucous membrane, the same proliferation of tissue, the same wasteful flux of melted or broken-down cells. But as we disguise a catarrh of the skin by a term drawn from a crude and antiquated metaphor, it is not surprising that our therapeutic analogies are confused or obscured. *Eczema* is not a bit more scientific than *staphyloma*, and carries with it no idea which is not more aptly expressed by the generic title *dermatitis*, with its many therapeutic suggestions and associations.

I propose to keep now to the subjects of eczema and pemphigus, well-worn as they may seem to be, because they illustrate two important principles in the pathology of the skin and its treatment. And after I had written most of this paper it was a pleasure to read Dr. P. H. Pye-Smith's thoughtful "Observations on the Various Forms of Superficial Dermatitis," contributed to the last number of *Guy's Hospital Reports*.

Now let us look at the morbid process called eczema, and try and study it without the trammels of academic formula. It is simple clinical truth when we delineate four grades of inflammatory action, which may be characterised as follows:—(*a*) the powdery desquamation of chronic passive erythema, or so-called *pityriasis;* (*b*) the active papillary hyperæmia, with enormous exfoliation, which constitutes *pityriasis rubra;* (*c*) the serous exudative inflammation which is called *eczema rubrum;* (*d*) the

purulent exudative inflammation which has been known under the names of *impetigo* or *porrigo*. Now are not these conditions bound together by an obvious pathological link? and if so, we should speak of them as dry and moist forms of inflammatory action, instead of borrowing titles for them from ancient languages, puzzling to the student and the unripe practitioner. Surely the last two forms ought to be bracketed together, for they differ only in the fact that the serum of the one has become pus in the other, owing to a more intense local action or a lower tone of general health. Essentially the process is the same; but underlying all the phenomena there must be a special vulnerability of skin, or readiness of the skin to be irritated. This may be developed to such a high degree that the skin seems a battlefield for neurotic energies and humoral poisons, almost justifying the new institution called a "dartrous diathesis."

A few preliminary words may be allowed on these type-forms of dermatitis, and some intermediate conditions.

I have seen only one case of extreme dry general dermatitis, commonly termed *pityriasis rubra;* but I watched this case (in 1869-70) from the beginning to nearly the end. The dermatitis was absolutely dry throughout; it was universal, and most severe on the front of the abdomen and of the thighs. The shedding of scales was enormous, and was a measure of that great albuminous waste which ended after some months of suffering in a fatal exhaustion. A medical friend thought with me that this was an example of acute psoriasis, showing that we did not then know the differential characters of what is now rightly styled "exfoliative dermatitis." In these cases there may be "weeping spots" on an unusually red area of skin; or a few scattered vesicles may appear, analogous to the local "boiling-over" of an intense lichen. Dr. Pye-Smith's second case (*loc. cit.* p. 270) is said to have been attended with such "free eczematous secretion" that I venture to doubt the correctness of classifying it as dry dermatitis at all.

The next point of interest to be noticed is the *occasional universality of serous dermatitis,* or quite as nearly so as many instances of the dry dermatitis group. Now yellowish crusts or small dingy scales may accumulate as the result of the

drying up of the serous secretion, not because the serum has become pus; and underneath crusts and scales more and more secretion may be poured out and pent up, oozing here and there, soon decomposing in hot and damp weather, and inoculating the adjacent healthy skin by soaking into its glandular apparatus. Up to a certain point, however, serous dermatitis should be regarded as clearly non-purulent for all therapeutic purposes. The feature of pathological importance is the extent of the "weeping" surface; if this be very great and the cutaneous flux considerable, the effect on the health may be severe at both extremes of life. And if there should be in addition a pruriginous or neurotic complication, the danger may become exceedingly grave.

Dr. McCall Anderson has remarked that *erythema circinatum syphiliticum* may become vesicular, with superficial ulcerations ringed in form, and approximately symmetrical.[1] I have lately seen quite a typical case in a man past middle age (almost old in look), and who had been under the care of my friend Mr. Harper, of Batheaston.

A few words on what Dr. Pye-Smith calls "chronic, dry, single-patch eczema, not itching, and lasting for years unaltered." A strong hearty gentleman, between fifty and sixty years of age, manager of a bank, comes to me occasionally from a town in Wiltshire, on account of one of these patches on the inside of the flexure of the right knee-joint. If I did not know his history, I should incline to look upon this little area of harsh infiltrated skin as a bit of a rather non-scaly psoriasis in an unusual place. And the effects of a special treatment (to be described presently) would justify the opinion that these patches of old non-secreting eczema are borderland territory impossible to be strictly classified, like fragments of one English county which have strayed within the limits of another.

I will speak now of the treatment of two forms of dermatitis (*eczema rubrum*, and old dry eczema), a subject which has had my attention for some years. In the pages of the *Practitioner* questions of therapeutics hold the first place. My experience

[1] I may mention here a good formula of Dr. Anderson's for irritable eczema—an ointment composed of subnitrate of bismuth and spermaceti, mixed with a little spirits of wine. Some of Dr. Liveing's published formulæ are very useful.

is drawn chiefly from cases in private practice; and these are often difficult to manage successfully, for the medical attendant must do a great deal himself, and spare no time and trouble in superintending the smallest details.

The case of Miss M. (a private patient of mine) furnishes a text for discussing the best way of managing red catarrhal dermatitis. She came to Bath in November, 1880, and had been under the care of Dr. Liveing during the previous summer in London. On her arrival here an old spotty eczema which she had long suffered from was confined to the legs, on which were many small irregular patches, sometimes a little hot and irritable. The general health was unsatisfactory. There was a history of old bronchitis; the constitutional tendency was distinctly gouty; and there was slight albuminuria. Much good was done by immersion twice a week in a tepid bath of the "Bath waters," and by the use of an excellent soothing application prescribed by Dr. Liveing, composed of vaseline and sweet almond-oil. She left Bath after a short stay, and returned at the end of 1881 with an outbreak of acute eczema as nearly universal as a skin disease can well be. There was much catarrhal bronchitis with emphysema, and the albuminuria was more marked. On the chest, abdomen, and back the eczema was weeping and irritable; on the thighs more inflammatory; and all over the legs the eruption was *eczema rubrum* in an almost malignant severity. Treating the disease on the upper parts of the body with vaseline and almond oil (the effect of which was incomparably good), the question was what were we to do with the legs?

Let me describe, firstly, two ways in which red weeping dermatitis ought *not* to be treated, although sanctioned by no mean authorities. Around a leg afflicted with this malady it was once the custom to lay strips of lint or rag soaked in an astringent lotion, and to put over these an impervious covering to keep them wet; yes, and the *leg* was kept wet too! For when after the lapse of a few hours the time came for a fresh "dressing," who can forget the steaming fetid mess in which the poor limb was bathed? Was there a possibility of healthy action being set up when we were stewing a leg in its own pathological juice (so to speak) by a method which is enough

to render the most healthy secretion morbid? Almost equally bad is the sprinkling of a moist red surface with dry powders of any kind, for they cake into dirty lumps, and are therefore insufficient for any protective purpose. It is almost unnecessary to say that ointments spread on linen or lint are never to be thought of.

What remains, then, to be done? Coleridge speaks of a painting as an intermediate something between a thought and a thing. In like manner the point to be aimed at in the present case is the application of a *tertium quid*—not a powder, not an ointment, but something which combines the virtues of both with the drawbacks of neither. This aim is fulfilled by a soothing fluid which holds an astringent powder in suspension, and equally diffused through it. Take half an ounce of oxide of zinc, four scruples of the best calamine, a fluid ounce of glycerine, and seven ounces of lime water. Add a little lime water to the dry powders, and mix into a paste; pour in more lime water, and stir in a mortar until the ingredients are well mixed. Then put the whole into a bottle, and add the glycerine last. Before using, shake the bottle well, and pour a little of its contents into a shallow open vessel, as a saucer. Take strips of lint or linen rag from two to three inches wide, and of different lengths, to correspond to the different diameters of the limb. Soak them in the lotion, taking up on their surface as much of the powder as possible. Carefully adjust these medicated strips over the affected part of the limb; and then around all wind evenly and with gentle pressure a bandage made from a calico of open texture, like what is used by farmers as a covering for cheese. This process must be repeated at first every morning and evening, taking care that the strips are well moistened with warm water before removal, although the glycerine in the lotion prevents them from ever becoming quite dry. Commonly this plan succeeds very well; the skin gets softer and less moist, soon only one dressing in the day is required, and in cases uncomplicated with visceral disease a cure is speedily effected. But when we have to deal with limbs swollen from cardiac dropsy, and a vesicular *erythema læve* arising out of this, we cannot do more than afford temporary relief.

To return to my patient, Miss M., whose case is the text of this therapeutic discussion. Mr. Ollerhead, of Minehead, a former medical adviser of this lady, came up and met me in consultation while I was carrying out the treatment described above. When all acute local symptoms had subsided then a place was found for a combination of lead and zinc ointments, or a mixture of acetate of lead and oxide of zinc with cerate of petroleum, which soon produced a dry, soft, glossy skin. The eruption on the back and the abdomen had nearly faded when Miss M. left Bath, which she did in February, 1882, much relieved in all respects.

Dry patches of chronic dermatitis, clearly eczematous in origin, are curable by the application of tar. But the tar must be weakened down by being mixed with astringent and soothing mineral powders. This is the essential qualification for the benign use of tar in any form of skin disease; and it is because this precaution has been often neglected that the external application of tar has been passed over and even disparaged. Of all the emollient materials proper to be mixed with tar, chalk is *facile princeps.* Finely levigated chalk should be strewed into melted lard in a stone jar, and thoroughly stirred until cold. Now the chemically antacid properties of this compound make it valuable,[1] but it is of great utility as a medium of potent medicaments which cure by a specific property *when used in a studied proportion, but irritate and inflame when that proportion is exceeded.* Now if the merest morsel of *Ung. Picis liquidæ* be added to chalk ointment, just sufficient to yield a brown smear of colour (and the eye is the best judge in such a matter), the infiltration and scaliness of an old dry dermatitis will gradually soften down; and, if assisted by the right auxiliary means internally, there will be nothing left in the long run but a pigmentary stain. In many cases

[1] Some "Notes on the Treatment of Ulcers" recently published in the *Practitioner* (January, 1883) by Dr. Whitson, describe several plans which appear both complex and painful; and Dr. Whitson may like to know how healing and soothing chalk ointment (spread on lint) is for every kind of ulcer, and how it practically supersedes all other applications when helped by proper bandaging. For an exposition of this method I refer to my work on the *Pathology and Treatment of Ulcers and Cutaneous Diseases of the Lower Limbs*, published by Messrs. Churchill in 1868.

the addition of a small percentage of *unguentum hydrargyri nitratis*, or of a few grains of the nitric oxide of mercury, is a material help. Do not these therapeutic facts show that one disease of skin-texture may glide into another? Subtle alliances of pathological kinship are suggested when we find that the remedy *par excellence* for the chief of squamous diseases may greatly alleviate the rough and almost ichthyotic specimens of infiltrated eczema which are met with on both extremities.

The late Dr. Tilbury Fox described an unusual form of *Lichen planus*, seen on the front and the sides of the leg. A patch may be as large as the palm of the hand; it is elevated, and feels and looks like a rasp, or a harsh warty growth. It is very amenable to local treatment.

And now let me say something about a case of an essentially neurotic disease, such as I believe pemphigus to be. It is not the fact of a disease being vesicular that makes it a neurosis; it must obey certain laws of coming and going, and show some orderly phenomena in its outward manifestations. The patient whose case has interested me so much during the last two years was born in February, 1862, and is the son of a Bristol merchant who resides in Bath; he wrote for me a careful history of his malady, and from it I make the following extracts. "In 1874 a few red spots first appeared on my hands, where, in about six days boils formed and filled with a clear watery fluid. These boils were pricked with a needle, and after a few days the under skin would thicken, and the outer skin dry and soon peel off, and I should be apparently well. From time to time these boils, which also formed about the lips and ears, broke out so often that they were considered as nothing extraordinary, and I was expected to grow out of them. A few bottles of mixture, such as were considered 'good for the blood,' were had of our family chemist. After my spots had appeared and disappeared a few times, our surgeon (we then lived in Cornwall) was consulted, and he was of opinion that I was in a growing state and had overheated my blood while playing football or cricket. He advised that I should not play or do anything that would heat me, and that I should keep in the same temperature as far as possible. The complaint was not kept away by the medicine which this surgeon supplied; the spots disappeared for

a while always to come forth again. At times my fingers were so swollen that I could not hold a knife or fork; if in the summer I exposed my swollen fingers to the heat of a hot sun, the irritation would be intense; and if in the winter I exposed my hands to the cold all irritation was allayed, but it always returned when I went from the cold outer air into a warm room. About January 5th, 1881, my spots reappeared with great severity after a longer absence than usual, and I consulted the same surgeon who had seen me on previous occasions. He warned me from the first that I should apply no ointment or drug outwardly to allay the pain; his medicine, he said, was intended to bring the spots out. By the 16th of January I had endured great pain, and there were traces of spots so late as the 20th of February. After a very short interval the spots showed themselves again early in March, and on the 5th of this month I first consulted you."

The sequel of this story is soon told. Exactly a year afterwards, my patient reported that there had been "no outbreaks" at all: only a few spots had appeared at intervals, but not such as to keep him from office duties. In January, 1883, he again reported that except for a short period during the preceding July, there had been no "spots" or "bladders" for a whole year; and he considered himself as "totally cured."

The treatment of this gentleman's disease was of the simplest kind. At first I applied to the fingers a combination of vaseline and lead ointment, adding subsequently a small proportion of *ung. hydr. nitratis dilutum*. But local remedies soon became unnecessary. Iron and arsenic were administered internally from the beginning, and with the best effect. Now and then these medicines caused an inconvenient alvine looseness; but from October 1881 to January 1883 they were taken daily with such regularity that if all the omissions were counted up they would not measure in time more than eight weeks.

Only a short comment is needed on this case. For seven years the nature of the disease was unsuspected; or if it were ever approximately diagnosed the therapeutic management of it was wholly inadequate and even mistaken. Mr. Jonathan Hutchinson, in his clinical lectures on pemphigus, relates some cases to show how little kindness and attention by themselves

will do for infantile pemphigus, if not aided by the special drug which alone can cure it. Children suffering from this disease are now and then sent to the Bath Mineral Water Hospital, apparently because other means have been tried in vain; but we have proved again and again that our thermal waters have no efficacy beyond any other alkaline lotion, and that without the internal use of arsenic our patients are not only not cured, but make no substantial progress towards a cure. It must be acknowledged, I think, that a strong testimony is afforded to Mr. Hutchinson's doctrine by the therapeutic history of the private patient which is recorded above. He was the victim of a malady obvious and even loathsome, very suggestive of something contagious, and it was at least a great hindrance to the practical duties of life. His medical attendant so missed his way as to confess that his treatment was intended to "bring the spots out;" and this illustrates one of the professional and popular fallacies about skin diseases. They are not to be "driven in," or, in other words, they are not to be cured lest worse internal troubles should follow. How many babies have been made miserable by this fanaticism, and been doomed to suffer a running dermatitis of the head or even of the whole body until the last stage of mal-nutrition has been reached, and death has nearly closed the scene.[1] Hebra laughs heartily at this grandmotherly scheme of physiological pathology. Do we ever say that we must not try and cure an old muco-purulent bronchitis lest the *sputa* be "driven inwards?"

There is a great divergence of medical opinion on the relative degree of the constitutional and local causes of dermatitis. One observer says of typical eczema that he is "quite unable to recognise its association with any other disease, or with any supposed diathesis, constitution, dyscrasia, or temperament."[2]

[1] Thus in the case of the infant child of some prosperous country people, enjoying the advantages of the purest air and food, a chronic and almost universal "moist tetter" produced an extreme degree of wasting. The substance of the best local medical advice was that the child would "grow out of it;" but meanwhile the little patient was not growing, but dying of its malady. A gradual cure was effected by an incessant varnishing of the skin with vaseline and sweet almond oil, and the internal use of cod-liver oil. This was in the early part of 1881, and I have just heard that the child has been well ever since.

[2] Dr. Pye-Smith, *loc. cit.* p. 214.

Another writer says that eczema "is always an expression of a diathesis, and that we can often best treat eczema by not regarding it as a disorder of the skin."[1] Common sense suggests that both these statements are too absolute. Sir Erasmus Wilson seems to hit the middle path when (in the Preface to his *Diseases of the Skin,* 5th edit., 1863) he speaks of the necessity of "combining the neuropathist and the humoropathist;" he contends for the importance of recognising the "constitutional origin of local disease," and the "delicate handling of a local disorder." Of the two errors that of wrongly "handling" the outward and visible disease is by far the greater. But experience confirms the practice of those dermatologists who think that the action of the kidneys ought to have special medical attention. It is certain that scanty urine and constipated bowels may baffle the most skilful local means.

A final word on the therapeusis of the Bath thermal waters. I never use them for the acute and chronic "moist tetter," for we have much surer and quicker methods of treatment; but for the drier forms of dermatitis bathing two or three times a week in the Bath waters is highly remedial, and is a valuable auxiliary to specific medicines by conferring on the skin a new nutritional power. This statement is more easily exemplified among private patients, as in hospital practice it is difficult to say how much of the therapeutic work is done by rest, better feeding, and more regular habits.

[1] Dr. Saundby, *Brit. Med. Journal,* December 23, 1882.

ON CARDIAC THROMBOSIS AS OCCURRING IN ACUTE DISEASE.

BY HENRY F. A. GOODRIDGE, M.D., F.R.C.P.

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Sir Joseph Fayrer, in the early numbers of this year's *Lancet*, and Dr. McAldowie, in the January issue of the *Journal of Anatomy and Physiology*, have recalled attention to the occasional occurrence during life of fibrinous coagula in the cavities of the heart and in the great vessels proceeding therefrom, in short, to the subject of cardiac thrombosis. Since the time when the so-called "polypi of the heart" were believed to be the cause of all the symptoms resulting from disease of that organ, there have been few subjects that have given rise to greater difference of opinion among authorities than the one before us. Whenever, indeed, we approach its consideration, we find certain questions presenting themselves which are apt to be very differently answered. To say nothing of the distinction between "true" and "false" polypi, or of that between fibrinous coagula of ante-mortem and of post-mortem date, but assuming at once that these concretions are of ante-mortem formation, what is their import? Are they, when they occur, mere incidents of the final struggle, results of a particular mode of dying, and thus included as it were in the article of death? or, are they concerned in any class of cases in any way or to any degree in determining the fatal issue? would death have occurred when it did occur in such cases without their presence? in other words, are they causes rather than consequences of this event? It is manifest that according to the answers we are prepared to give to these questions will be our views of the significance and

importance of cardiac thrombosis, and our attitude as regards treatment, preventive or remedial, in any given case. And thus there come to be two opposite parties : the one represented by Dr. Richardson and the writers above named, who consider the formation and deposition of fibrin in the chambers of the heart as of supreme clinical importance ; the other who, following in the wake of Morgagni, look upon such occurrence as a mere secondary phenomenon and of no practical account. Sir Joseph Fayrer and Dr. McAldowie have treated the subject in its relations to surgical operations and chronic disease ; I propose now to glance at it in reference to acute disease, and shall draw upon my hospital records for the purpose of illustration.

Case I.—L. I., æt. 27, a pattern-maker at an iron-foundry, was admitted into hospital, *May 10th*, 1864. He had had two previous attacks of "rheumatic fever;" but stated that his health was good until three weeks before admission. He was then seized with nausea and vomiting, which, soon passing off, left him feeling unwell, but not so as to prevent him doing his work. On May 4th, symptoms of acute articular rheumatism supervened which at once disabled him. All his joints in turn became affected ; his right hand had suffered least. He had had no treatment. *On admission,* his joints being all more or less tender, he complained chiefly of pain in the cardiac region on deep inspiration. A faint but distinct pericardial friction sound was heard at the mid-sternum ; there was also an obscure systolic murmur at the apex. His pulse was 128, of moderate force and volume, his respirations were 32, his alæ nasi moving with them. His tongue was furred and dryish ; his appetite was impaired ; his bowels were confined ; his urine was acid, of sp. gr. 1·023, and non-coagulable with heat and nitric acid ; his skin was perspiring. There were no evidences of any pulmonary affection. He was ordered to have a dose of house medicine immediately, eight leeches applied to the præcordia, Opii gr. i. night and morning, and Potass. Bicarb. ʒss. ex Aq. Menth. Pip. every three hours—to be put into flannel and blankets, with cotton-wool applied to the affected joints, and for diet to have milk and beef-tea.

May 11th.—The bleeding from the leech-bites had been excessive, so that the resident medical officer was summoned in

the night to stop it. The patient was relieved of his pains, and the friction-sound was now absent. His pulse was 104 and smaller; his bowels had not acted.

May 12th.—The friction sound had returned; his pulse was weak; his bowels were still unopened. He was ordered to have the house medicine repeated with an ordinary aperient enema to follow, to take the alkaline draught every four hours only, and to have six ounces of brandy in the course of the twenty-four hours.

May 13th.—He had had some delirium in the night. The friction sound was hardly perceptible; his pulse was 104, vibratile, but stronger than preceding day; his respirations were 24; his bowels had been well opened; his urine was now alkaline.

May 14th.—At 10.15 A.M., without any premonitory change being observed, or any movement or other exertion apparent on his part, the patient suddenly expired; indeed, the nurse, who the moment before was at his bedside, had gone but a few paces therefrom when the event occurred.

On post-mortem inspection, the pericardium was found to be universally adherent, the adhesions, which were nowhere dense or tough, being towards the base of the heart, of quite recent formation; there were no external adhesions. On opening the left ventricle, a long firm coagulum of decolorised fibrin was found extending from the mitral orifice into the aorta. It was adherent to the mitral valve, and had filaments intertwined with its chordæ tendineæ. There were some small bead-like excrescences on the aortic segment of this valve, and the fibrinous coagulum on its removal exhibited an accurate cast of the same; it presented a constriction at the part corresponding to the aortic orifice, and reached nearly as far as the giving off of the innominate branch, tapering towards its termination; it was loose in the interior of the vessel. The sigmoid valves were healthy. The right cavities were occupied by *coloured* coagula. There were some old adhesions of the left lung, but the other organs were fairly healthy.

CASE II.—I. G., æt. 22, a carpenter, was admitted *December 6th*, 1870. He inherited paternally a tendency to gout; was weak during childhood, but about 15 his health improved.

At 18 he was troubled with tapeworm, and had been so more or less since. He had been as well as usual previously to December 2nd, when in the evening he was seized with shivering, which continued off and on through the next day. During the night of December 3rd, pain came on in his left foot, which prevented his sleep and obliged him to remain in bed. The following night his right foot also became painful, and subsequently other parts had suffered. He had had loss of appetite since December 4th. *On admission* he looked pale and prostrate. The ankle and ball of great toe of left foot were considerably swollen and very tender; there was also tenderness in right foot and both knees; the phalangeal joints of fingers were similarly affected, but the wrists, elbows, and shoulders were free. He complained of pain in the cardiac region on taking a deep breath, and said he had had some palpitation the preceding night. A systolic murmur was observed at the apex of the heart, also at its base. His temperature in axilla was 103° Fahr.; his pulse was 112 rather sharp; his respirations were 26; his tongue was moist, with whitish fur on its dorsum; his bowels were now open, an aperient having been required. He was ordered to take Potassæ Nitr. gr. xv + Pot. Bicarb. ℨss. ex Aq. Menth. Pip. every four hours, and Quiniæ Sulph. gr. v. in form of pill night and morning, and to be put into flannel and blankets with cotton-wool applied to the affected joints and the præcordial region, and for diet to have milk and beef-tea.

December 7th.—He had had a very disturbed night. The same sharp pain in the cardiac region continued, it caused him a feeling of faintness. The systolic murmur at base was now inaudible. His temperature was 101·2°; his pulse was 120; his respirations were 28; his bowels were open; his urine was 1½ pint in the 24 hours, of sp. gr. 1·020, alkaline, and free from albumen; his perspiration was copious. He was ordered to take the draught every six hours only, and to have at night instead of the second quinine pill, Pulv. Ipecac. Co. gr. x.

December 8th.—He slept a little the early part of night, but his pains were now increased, in the hands especially. His temperature was 101·9°; his pulse was 112; his respirations were 34; his bowels were open; his urine was 2½ pints, of sp. gr. 1·025, and slightly acid; his perspiration was copious.

December 9th.—He had had a restless night, with a good deal of pain in the cardiac region. To-day the pain was more in his joints. His temperature was 101·8°; his pulse was 108; his respirations were 44 and somewhat irregular in rhythm; his urine was 2 pints 16 ounces, of sp. gr. 1·022, and acid. He had an anxious expression, and from day to day a peculiar agitation of manner was noticeable in him. The draught and pill being continued, four minims of the hypodermic solution of morphia, along with two minims of the solution of the sulphate of atropia, were ordered to be subcutaneously injected at bedtime.

December 10th.—He slept better; perspired profusely. The most pain now was in his left hand; that in the cardiac region was less. There was a distinct harsh systolic murmur at base of the heart, that at apex remaining; no friction-sound was detected. His temperature was 102·5°; his pulse was 126; his urine was nearly 3 pints, of sp. gr. 1·018, and acid, it contained a trace of albumen.

December 11th.—He slept pretty well; the pain in the cardiac region had still further abated, so that he was able to breathe more freely. There was no friction-sound. His temperature was 100·9°; his pulse was 120; his respirations were 32; his bowels were not open; his urine was 3 pints, of sp. gr. 1·014, and acid; it deposited in the test-tube $\frac{1}{12}$th of albumen. He was still perspiring profusely. He was ordered to have an aperient enema and to continue the other treatment.

December 12th.—The subcutaneous injection of morphia and atropia (as prescribed on the 9th) having been repeated at nine last evening, the patient slept until about 1 A.M., when he suddenly woke up in a state of great alarm with a sense of impending death; shortly afterwards his breathing became embarrassed; the resident medical officer, who was summoned, found him almost pulseless at the wrist, and a few minutes later, or about half an hour from his waking up, he died.

On post-mortem inspection, there were found some slight traces of recent pericarditis. On opening the right ventricle, a long, firm, decolorised, fibrinous, coagulum was seen occupying its cavity and extending thence into the pulmonary artery and its primary divisions, which it appeared very completely to block.

It was found to yield to gentle traction in removing it from the vessel, but it was pretty firmly attached to the columnæ carneæ of the ventricle. It was solid and homogeneous in texture, and it presented an exact impress of the pulmonary sigmoid valves.

The left ventricle also had within it a pale fibrinous coagulum entangled among the chordæ tendineæ of the mitral valve; it was much smaller than that of the right ventricle. The aortic valves were perfectly healthy, but there were two or three small red vegetations on the mitral valve. The liver was in a state of congestive enlargement; also the kidneys were congested. Nothing important was found elsewhere.

CASE III.—E. V. æt. 22, a sempstress, was admitted *August 6th* 1872. She was too ill to give any account of herself, and the history collected from other sources was very imperfect. It was ascertained, however, that about a fortnight before admission she had suffered from sore throat and swollen glands in the neck, and that within the last week the skin had peeled off her hands; also it appeared that twelve months previously she had had some form of primary syphilis. On August 3rd, her breathing up to that date having been quite free and undisturbed, she began to be troubled with dyspnœa, particularly on movement or exertion; this had gradually increased in intensity during the following days; and *on admission*, August 6th, 2 P.M., it amounted to complete orthopnœa. She was now in a state of extreme distress and restlessness; she pointed to the scrobiculus cordis as the seat of her oppression; and she expressed herself as though dying. Her face was pallid and slightly dusky, and her lips and tongue were somewhat livid; her brow and surface generally were cool and bedewed with perspiration; her respirations were 56 in the minute, the alæ nasi moving conspicuously with them; her radial pulse was so small and imperceptible that it could not be counted; the heart's apex could be felt, though obscurely, and in about the normal position, and its pulsations were 172 in the minute; its sounds were indistinct, but its action was so tumultuous and the patient so restless that any satisfactory auscultation of the cardiac region was impossible; there was some little dulness with slight bronchial breathing at the right base, otherwise exaggerated breath sounds were heard over the entire of both

lungs and particularly of the left. There was no tenderness of her abdomen and her bowels were regular; her urine, however, was dark-coloured, of sp. gr. 1·019, loaded with albumen, and under the microscope was found to contain large quantities of red blood discs, and renal epithelium with numerous small hyaline casts; her catamenia were regular. There were several blotches over her body, apparently the relics of rupia of no distant date. She had no head symptoms. Her restlessness was too great to allow of any observations with the thermometer. She was ordered to have sinapisms applied to the scrobiculus cordis with hot-water bottles to her feet, and to take Ammoniæ Carb: gr. iii + Sp: Chloroformi. ℳ xx ex Aq: Camph: ℥j every second hour, and half an ounce of brandy out of a little hot milk every half hour.

6 P.M.—Her heart's pulsations were 180 in the minute; her hands and feet, which were not œdematous at 2 P.M., had now become so; her general condition was unrelieved.

August 7th.—At 1.15 A.M., without any further change in her symptoms, she expired.

On post-mortem inspection (at twelve hours' interval). There was a fair amount of fat in the integuments. The right pleura contained from three to four pints of serous effusion; in the left also was effusion, but in smaller quantity; in neither were there any adhesions, false membrane, or lymph. The *lungs* were both moderately collapsed; they were somewhat condensed, but nowhere solidified; their texture was dryish and exsanguine. In the pericardium was a small quantity of serous fluid; it was free, as was the membrane itself, from inflammatory products. The *heart* was of about normal size. On opening the right ventricle the cavity was found all but empty, only a coloured, stringy, coagulum was in it. The right auricle, on the contrary, was occupied by an irregularly shaped, colourless, fibrinous mass, which adhered closely to the wall of the cavity and especially to that of the auricular appendix, and seemed very completely to block the tricuspid orifice; off-shoots were intertwined with the chordæ tendineæ of this valve. On removing the mass, a long process of it was observable which was found to be tubular, admitting a No. 4 catheter to pass within it for a couple of inches; this evidently occupied the superior vena cava. A

constriction marked off that portion which belonged to the auricula, and impressions of the musculi pectinati were plain upon the surface of the same. Several small coloured coagula were attached to the mass. Its structure was found to be distinctly stratiform in concentric layers, but any free central channel beyond or besides that already noticed could not be detected. The left auricle with its pulmonary veins was full of dark grumous blood; a single, colourless, fibrinous band was free in the cavity of the left ventricle. There was incipient atheroma of the aorta. There was some serous effusion in the peritoneum. The *liver* was enlarged; it weighed 4lbs. 6oz.; dripped with blood on section, and was a little coarse in texture. The *kidneys* were enlarged; they weighed together nearly 13oz.; both were whitish in their cortical portions, and had more or less congestion of their medullary cones. Microscopically, they were found to have their uriniferous tubes stuffed with granular cloudy epithelium. The head was not examined.

Of these three cases now given, the last, No. III., is clearly the least open to question as to its nature. The close correspondence between the progressive development of the dyspnœa, præcordial distress, and other symptoms, and the obviously gradual growth by accretion of the laminated fibrinous mass in the right auricle with *pari passu* obstruction of the cavity of the same, would seem to admit of but one explanation. It may not be so easy to say what started the coagulation, whether this was autochthonous or embolic, though its starting-point was doubtless in the auricula. There was good evidence to show that the patient had shortly before suffered from scarlet fever, upon which nephritis ensued; and two conditions which are recognised as favouring the formation and deposition of fibrine, viz. deficiency of red blood discs and diminished density of blood serum, were probably both present in her case.

Cases I. and II. occurred in the course of acute rheumatism, a disease characterised by an increase in the amount of fibrine in the blood. In both cases also were cardiac complications enfeebling the action of the heart as well as providing favourable spots for the deposit of fibrine. In No. I., moreover, it is possible that loss of blood may have co-operated, though in recollection of the copious blood-letting formerly practised in

such cases without any immediate bad results ordinarily ensuing,[1] this seems hardly probable. On the other hand, it is noteworthy that both patients had been treated with alkalies and their urine had been rendered alkaline, No. II.'s, however, not remaining so. Now, whereas from the first onset of symptoms to death there was in No. III. an interval of about three days, here the event was sudden. In No. I. it was instantaneous; not a word of complaint from the patient, who was to all appearance perfectly quiescent on the occasion, nor any change observed by the nurse, had given the note of warning. Life was extinguished in a moment. He had adherent pericardium, and though by no means in its most aggravated form, this doubtless contributed with the endocarditis present to impair considerably the contractile power of the cardiac muscle. In connection with the endocardial inflammation, it is to be observed that, while the right cavities were occupied by coloured coagula, a long, firm, coagulum of decolorised fibrine was found in the left ventricle extending into the aorta, which had the aortic segment of the mitral valve for its base of attachment, and was accurately moulded to the characteristic bead-like prominences or vegetations that were upon its surface. Little caps of fibrine, as is well known, usually surmount these bodies. The explanation suggests itself, that in this instance the deposition exceeded its ordinary limits, and that, coalescence ensuing, under the play of the blood current a flattened band was gradually formed, which, at first perhaps floating loosely, would, as it got prolonged into the aorta, become a fixed obstruction at this orifice; against such increased resistance the weakened ventricle would be powerless, and in a moment its action might be arrested, and fatal "asystolie" supervene. The rigor mortis probably cleared the cavity of its other contents.

Looking at No. II. in the light of No. III., what is it but the latter case more rapidly evolved, the difference depending upon the different situation of the block? In No. III. this was in the right auricle and at the tricuspid orifice; whereas in No. II. it was in the pulmonary artery and its primary divisions, the

[1] The case of Andral's negro, however (*Clin. Méd.* 4ème édit. tome iii Obs. II.), would suggest that similar events to those under consideration in the text did now and then occur after such treatment, *post hoc* if not *propter hoc.*

lumen of which would of course in comparison be rapidly occluded. Clearly the mode of death was essentially the same in the two cases. The distinguishing point in No. I. was precisely what might have been expected from the influences at work being localised in the left ventricle and at the aortic orifice.

Cardiac thrombosis, then, is regarded as the determining cause of death in these instances, the thrombosis itself arising [1] under the conjoint operation of cardiac weakness and an hyperinotic state of blood. The importance of the cardiac weakness as a coefficient is fully recognised; but the special facts and features of the cases, particularly when looked at side by side of one another, forbid, it is contended, the notion that these patients had their lives thus abruptly brought to a termination by cardiac weakness alone, and that the fibrinous coagula were of mere "post-mortem" or "in articulo" formation.

Assuming the correctness of the view entertained, the practical lessons are obvious. (1) To maintain the strength of the heart, a task best accomplished by subduing the inflammation, endocardial, pericardial, or both, which is the direct source of its weakness. (2) To check the abnormal tendency to the formation and deposition of fibrine, and, when their presence is diagnosed and time is allowed, to attempt the removal, by solution or absorption, of concretions already formed. At present our knowledge avails but little for the first of these purposes, and next to nothing for the second; if we only recognise, however, the insidious character of the processes concerned and their formidable results, and direct our aims and efforts accordingly, it may be hoped that we shall not long remain so helpless in dealing with them.

[1] The remarks here and to the end apply for the most part to Cases I. and II.

THE CHEMICAL NATURE AND PHYSIOLOGICAL ACTION OF NITRO-GLYCERINE.

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SINCE the discovery of nitro-glycerine by Sobrero in 1847, who also recognised its great physiological activity, this body has more engaged the attention of the engineer than of the pharmacologist. There exist, it is true, the records of stray observations on its physiological action by Field, Fuller, Harley and others, and accounts of more systematic investigations by Pelikan, Demme, Albers, Eulenberg, Onsum, and Weiber, but, with the exception of a comparatively recent research by Lauder Brunton and E. S. Tait—and even this research is admittedly incomplete—none of them is of sufficient precision and extent to furnish a perfect description of the effects of nitro-glycerine. The object, however, of the present paper is not so much to add to the observations of these investigators, as to account for the physiological action of nitro-glycerine from a consideration of its chemical characters.

All who have to any extent studied the action of nitro-glycerine agree in representing it as an extremely active substance. This, having regard to the chemical constitution of nitro-glycerine, is very remarkable. For nitro-glycerine is a compound ether—nitrate of glyceryl, $C_3H_5{\cdot}3NO_3$—whose acid and base, when taken separately, or in other combinations, have a very slight activity as compared with that of nitro-glycerine. For example, the nitrate of potash or the nitrate of ethyl is a mildly active body, and so also is chloride or sulphate of glyceryl.

Equally remarkable with the intensity of the action of nitro-glycerine is the nature or quality of the action. For nitro-glycerine in its relations to the organism behaves in a manner

exceedingly like that of a nitrite. No one who is acquainted with the physiological action of nitrite of potassium as described in Reichert and Weir Mitchell's paper,[1] a *résumé* of which I have already given in this journal,[2] or with the action of nitrite of amyl, as portrayed in numerous papers, one of the most important of which is that by Lauder Brunton,[3] can read Lauder Brunton and Tait's account[4] of the action of nitro-glycerine without being struck by the great resemblance which the action of the last of these substances bears to that of the other two. All three differ from each other in the intensity of their action; but it is impossible, so far as present investigations go, to point out any essential difference in the nature of their action. A short comparison of their effects will make this quite apparent. It will be sufficient for the purpose to compare the action of nitro-glycerine with that of nitrite of potassium, as Reichert and Weir Mitchell have already established the identity of the action of the latter with that of nitrite of amyl. For the data required for the comparison, I am almost entirely indebted on the one hand, to the able and elaborate paper of Reichert and Weir Mitchell, and, on the other hand, to the admirable, though brief, paper of Lauder Brunton and Tait.

Administered to frogs, nitro-glycerine causes at first great restlessness, lasting for a few minutes, quickly giving place to a lethargic or semi-narcotic state, in which the animal can be pushed along the table without jumping. This is followed by a succession of tetanic convulsions, accompanied by twitchings of the toes, which gradually become weaker until the animal dies.

The action of nitrite of potassium is almost precisely similar, the only difference being that the tetanic convulsions do not form, although present, so prominent a feature. But this may be readily accounted for by a difference in the kind of frog used in the various investigations, or in the condition of the frog. For, it is well known to experimental pharmacologists

[1] Reichert and Weir Mitchell, *Americ. Journ. of Med. Science*, vol. lxxx. (1880), p. 158.

[2] March, 1883.

[3] Lauder Brunton, *Ludwig's Arbeiten*, and *Journ. of Anat. and Physiolog.*, 1871, p. 2.

[4] Lauder Brunton and E. S. Tait, *St. Barth. Hosp. Reports*, vol. xii. p. 140.

that the kind and condition of the frog greatly influence the appearance of convulsions.

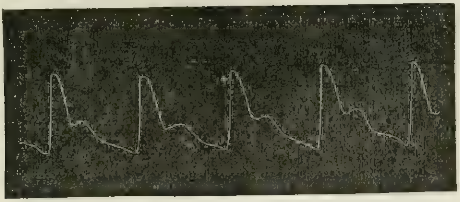
In mammals, nitro-glycerine produces little or no primary excitement. The animal becomes rapidly depressed, and the pulse and respiration become greatly increased in frequency, the respiration assuming a violent panting character. Voluntary motion becomes frequently paralysed, and reflex motion almost entirely so, and convulsions are extremely rare. Finally, the pulse and respiration become very slow, and death occurs from paralysis of the respiration.

This is an accurate picture of the action of nitrite of potassium on mammals—exact in every detail.

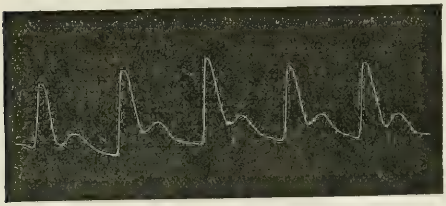
If the comparison be continued into the action of nitro-glycerine and the nitrite on the various organs and tissues of the body, it will be found, that both of them paralyse the motor and sensory centres of the spinal cord; that the tetanic convulsions, which occur especially in frogs, are cerebral in origin; that they both paralyse the voluntary muscles; that they at first quicken the action of the heart, and afterwards make the heart go slower, finally paralysing it; that they diminish the cardio-inhibitory power of the vagi; that they greatly diminish the blood-pressure; that they primarily stimulate the respiratory centres, afterwards paralysing them; and that they both cause the blood of an animal poisoned with them to assume a characteristic chocolate colour. Further, both of them, as Lauder Brunton and Tait point out, increase rather than lessen the oxidation of guaiac by vegetable solutions. I have also compared their action on the pulse as observed by means of the sphygmograph, and find that it is the same for both, and similar in kind to that of nitrite of amyl. A glance at the accompanying tracings, all of which were taken from the radial artery of my left wrist, will prove that they both diminish the tension of the pulse. The quantity of nitro-glycerine taken was two drops, or about one minim, of a one per cent. alcoholic solution, and the dose of the nitrite—the nitrite of sodium—was three grains. I have added a third tracing of the effect of the nitrite on the pulse, taken after the administration of a further dose of six grains, which shows extremely well the action that is so characteristic of nitrite of amyl, and which, therefore, affords additional proof of the

correctness of Reichert and Weir Mitchell's conclusions, that nitrite of amyl and nitrite of potassium are identical in action.

A.—Showing the Action of Nitro-glycerine.[1]

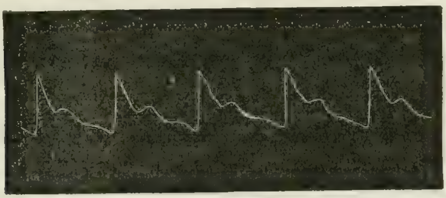


1.—Before administration of nitro-glycerine.

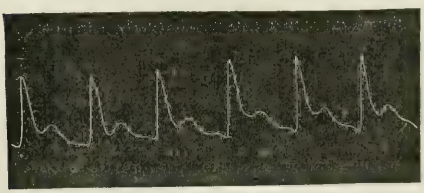


2.—Six minutes after administration of 2 drops of 1 per cent. solution of nitro-glycerine.

B.—Showing the Action of Nitrite of Sodium.

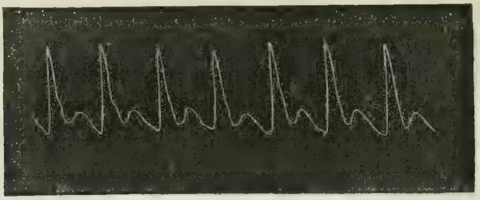


1.—Before administration of nitrite.



2.—Twenty-three minutes after administration of 3 grains of nitrite.

Dr. Murrell, to whom we are indebted for the introduction of the most important application of nitro-glycerine, viz., in angina pectoris, gives in his work on *Nitro-Glycerine in Angina Pectoris*, a series of sphygmographic tracings by Dr. Fancourt Barnes, exhibiting comparatively the action of nitro-glycerine and of nitrite of amyl, which prove that these substances have a similar action on the pulse.



3.—Thirty minutes after administration of a second dose of six grains, which was taken half an hour after first dose.

Further, nitro-glycerine and nitrite of potassium agree in producing throbbing in the arteries of the body, and a certain degree of flushing, although the former acts more powerfully in these respects than the latter.

The only conclusion to be drawn from this comparison of the physiological action of nitro-glycerine with that of a simple metallic nitrite, is that the action of both is essentially the same, at least in kind; for they differ greatly in the intensity of their action, one-hundredth of a grain of the former producing often a more violent impression on the organism than two or three grains of the latter.

This conclusion as to the similarity of their action receives also the support of clinical experience. For example, they seem to act alike in angina pectoris,[1] and they have both been used with good effect in some cases of epilepsy;[2] and, if we are unable to further extend the therapeutical comparison it is simply because the alkaline nitrites have been so short a time in employment as medicinal agents, that sufficient time has not yet been allowed for a trial of them in other diseases.

As, therefore, the physiological and therapeutical action of nitro-glycerine, nitrite of potassium, and nitrite of amyl is apparently the same for each, the suggestion naturally occurs that they contain some substance in common, to whose presence their characteristic action is due. As nitrous acid is common to nitrite of potassium and nitrite of amyl, we at once conclude, seeing that their bases differ so widely in their action, that the acid is the characteristically active parts of these salts. But nitro-glycerine is not a nitrite: all chemists agree in representing

[1] Matthew Hay, *Practitioner*, March, 1883.

[2] Reichert and Weir Mitchell, Ralfe, and Saundby, referred to in same paper.

it as a nitrate of glyceryl, with the exception of the editors of Watt's *Dictionary of Chemistry*,[1] who speak of it as a nitro-substitution compound, but this is evidently an error. Although chemists are agreed as to its constitution, it at first occurred to me, purely from a consideration of its physiological and therapeutical action, that, as the analyses of the nitro-glycerine, on which its constitution is founded, are so imperfect and unsatisfactory, owing to the difficulty of working with the substance on account of its great explosiveness, there was a possibility of nitro-glycerine being a nitrite and not a nitrate. The acids of compound ethers do not as a rule exhibit their customary reactions, as they would if combined with metallic bases, until the acid has been forcibly dissociated from the alcoholic radical by means, most commonly, of an alkali: for example, acetate of ethyl (acetic ether) gives no red colour with ferric chloride, as acetate of potash does. And I considered it possible that, although the nitrous acid of the nitrites derived from the primary alcohols gives its usual reactions as readily as if it were merely combined with a metallic base, yet the nitrous acid of ethers obtained from certain poly-atomic alcohols, as glycerine, might behave towards re-agents like the acetic acid of acetic ether, and thus have escaped detection. The usual test, and one of great delicacy, for nitrous acid, is to add a small quantity of a thoroughly well-boiled starch solution, in which a little iodide of potassium has been dissolved, to the solution containing the nitrous acid, when a deep blue colouration will occur, even if there be only the merest trace of nitrous acid present. If the nitrous acid be in the form of a nitrite, it is necessary to add a little dilute sulphuric acid in order to liberate the nitrous acid.

When I applied this test, with the addition of sulphuric acid, to an alcoholic solution of nitro-glycerine I failed to obtain any blue colour. I then proceeded to decompose the nitro-glycerine with potash previous to applying the test, and obtained abundant evidence of the presence of nitrous acid, the fluid becoming densely blue on the addition of the starch re-agent. The amount of nitrous acid present appeared so large that I at first concluded that nitro-glycerine is not a nitrate but a nitrite,

[1] Watt's *Dictionary of Chemistry*. Art. "Nitro-glycerine."

and that the statement universally met with in chemical books, and upon which the constitution of nitro-glycerine is chiefly based, viz. that nitro-glycerine is decomposed into nitrate of potash and glycerine by the action of potash, is false. A further extended and careful examination of the composition of nitro-glycerine and of its decomposition products has considerably modified my first impression. From a large number of chemical analyses, which will shortly be published elsewhere, I have found that nitro-glycerine is really a nitrate of glyceryl, but that, in the course of its decomposition by an alkali, two-thirds of its nitric acid are reduced to nitrous acid and unite with the alkali to form a nitrite, whilst the remaining third is set free without reduction and forms nitrate of potash; and it is a curious fact that no glycerine at all is formed, the whole of it becoming oxidised to other substances by the oxygen, which the two-thirds of the nitric acid lose in their reduction. This formation of a large quantity of nitrous acid in the alkaline decomposition of nitro-glycerine is of the greatest interest in connection with the physiological action of that ether, as it at once offers a very satisfactory explanation of the strong similarity of action which nitro-glycerine bears to nitrites: we have only to suppose that the nitro-glycerine undergoes a like decomposition in the presence of the alkaline constituents of the blood and other alkaline fluids of the body. In support of this supposition I have ascertained that an alkaline carbonate, as carbonate of potash or soda, decomposes nitro-glycerine with equal completeness, if with less rapidity than the corresponding alkali, and decomposes it with the production of exactly the same proportion of nitrous acid. If an alcoholic solution of nitro-glycerine is mixed with an alcoholic solution of caustic potash, decomposition instantly occurs with the evolution of heat and is completed almost at once; if the solutions be weak, it is necessary, in order to have perfect decomposition, to heat the mixture to the boiling point. If the carbonate of potash is substituted for the alkali itself, the decomposition is not completed until the fluid has been heated for about ten minutes over the water-bath, although, immediately on the addition of the carbonate, nitrous acid is formed. In order to test the decomposing power of an alkaline

carbonate under conditions approximating to those of the blood, I mixed a saturated solution of nitro-glycerine in water—containing about 1 in 800—with a small quantity of a solution of carbonate of soda, so that the mixture contained about 0·2 per cent. of the exsiccated carbonate, a proportion somewhat similar to that of the blood, and I placed the fluid in a digesting oven at a temperature of 40° C. Within one minute after mixing there was distinct evidence of the presence of nitrous acid on testing in the usual manner. At the end of ten minutes the total nitrous acid was estimated, and from its amount it was calculated that a little less than one-tenth of the nitro-glycerine had been decomposed.

To assure myself still further of the probability of such a decomposition being effected by the blood, I made the following experiment. I killed a cat, and quickly afterwards removed its blood and defibrinated it. I then mixed 10 cc. of the blood with 10 cc. of a 0·1 per cent. solution of nitro-glycerine in water, and digested the mixture for forty minutes in an oven whose temperature varied from 40° to 45° C. Immediately afterwards the fluid, which had now assumed a very distinct chocolate colour, was transferred to a dialyser suspended in distilled water at 40° C. After it had dialysed for one hour, a distinct trace of nitrous acid or a nitrite was detectable in the water, proving that the blood had decomposed the nitro-glycerine. Another portion of the water was heated with a little potash in order to decompose the nitro-glycerine, if any had diffused out and were present, and the usual test for nitrous acid was then applied without obtaining a much more distinct blue colour than previous to heating with potash. It would appear, therefore, that nearly all the nitro-glycerine had been decomposed by the blood, although the quantity of the nitrous acid in the diffusate, even after dialysis had continued for twenty-four hours, represented only a small proportion of the nitro-glycerine. The nitro-glycerine is, therefore, not only decomposed by the blood with the formation of nitrous acid, but the nitrous acid is used up by the blood for the oxidation of some of its constituents, especially of its hæmoglobin. This probably occurs whether a nitrite is added directly to the blood outside the body, or whether it is injected into the circulating

blood; for, as a rule, none of it appears as nitrous acid or a nitrite in the urine.

Further, I have been able to demonstrate that nitro-glycerine oxidises the hæmoglobin of the blood in apparently the same manner as a nitrite does. I digested for half an hour, at 40° C., 5 cc. of freshly-drawn defibrinated cat's blood with 10 cc. of a 0·1 per cent., or almost saturated, watery solution of nitro-glycerine, to which was afterwards added 1 cc. of a 2 per cent. alcoholic solution of nitro-glycerine, in order that the mixture might contain as much nitro-glycerine as it was likely to dissolve. At the same time, and using the same blood, I digested other 5 cc. of the blood with 10 cc. of a 0·07 per cent. solution of nitrite of sodium, a quantity of the nitrite which contains as much nitrous acid as the weak solution of nitro-glycerine would yield when decomposed; I afterwards added 1 cc. of a 1·5 per cent. solution of the nitrite.

Both fluids assumed a distinct dirty, or chocolate-brown, colour, particularly the fluid containing the nitro-glycerine. The original red colour of both was restorable by the addition of a reducing agent, as sulphide of ammonium, proving that the brown colour was probably due to oxidation of the hæmoglobin. This was confirmed by a spectroscopic examination of the fluids. It has been well known since the publication of Giacosa's observations [1] that nitrites do not form a compound with the hæmoglobin as Gamgee [2] believed, but that they oxidise the hæmoglobin to the form of methæmoglobin. The spectrum of this body is especially characterised by the presence of a very well-marked absorption band in the red, between the lines C and D; the spectrum is otherwise like that of oxy-hæmoglobin, excepting that the absorption band in the green is broad and extends towards F. This was precisely the appearance of the spectrum of both the nitrite and the nitro-glycerine fluids. In making this observation I had the assistance and valuable confirmation of Professor Rutherford, who very kindly granted me the use of his spectroscope. I emphasise this observation the more because Brunton and Tait [3] were not able to discern

[1] Giacosa, *Ztschrft. f. physiolog. Chemie.* Bd. iii. S. 54.

[2] Gamgee, *Philosoph. Transact.* 1868, p. 589.

[3] *Op. cit.* p. 145.

any difference between the spectrum of nitro-glycerine blood and of normal blood. These investigators either used, as they themselves suggest, a faulty instrument, or the blood was not sufficiently digested with the nitro-glycerine.

From a consideration of these various observations and results—the general identity of the physiological and therapeutical action of nitro-glycerine with that of nitrites, the formation of nitrous acid as a decomposition product of the action of alkaline fluids, including the blood, on nitro-glycerine, and the similarity of the action of nitro-glycerine on hæmoglobin to that of nitrites—we are, I venture to think, warranted in concluding that the activity of nitro-glycerine is due to the nitrous acid which is formed by its decomposition within the body.

If this conclusion be held as correct, the question at once arises as to why nitro-glycerine should be so very much more powerful in its action than a simple salt of nitrous acid: one-hundredth part of a grain of nitro-glycerine will produce a more powerful impression on most people than five grains of an alkaline nitrite. The explanation is probably this. A nitrite is freely decomposed by the acid of the stomach, nitro-glycerine is not; and the nitrous acid set free from the former is, in contact with water, decomposed with the formation of nitric acid and nitric oxide. This decomposition occurs the more readily if organic matter be present, as it is, in the stomach. In an acid medium nitrous acid is an extremely unstable body, whilst in an alkaline medium it is comparatively stable. The result is that only a small proportion of the nitrite of sodium administered is absorbed as such and in an active form. The quantity will be greater the less are the acidity and the bulk of the contents of the stomach. It is possible to conceive of certain conditions in which practically none of the nitrite is absorbed, as, for example, one or two hours after a meal, when the stomach is full of a strongly acid mixture, and especially if the absorptive power of the gastric mucosa is impaired through the existence of chronic catarrh, as is often the case. On the other hand, if the stomach is empty, and if the little mucus within it possess its usual alkaline reaction, the nitrite may be entirely, or very largely, absorbed as such. The usual condition, however, is that the stomach contains an

acid fluid, hence the decomposition of the nitrite and its lessened activity.

It is quite otherwise with nitro-glycerine. It is not acted on by the acid of the stomach, and the whole of it, therefore, passes into the blood unchanged. If nitro-glycerine be digested with a 0·1 per cent. solution of hydrochloric acid, or even with a much stronger acid, it undergoes practically no decomposition, as I have several times proved. None of the latent power of the nitro-glycerine is therefore lost, previous to its absorption into the blood. A smaller dose of nitro-glycerine may accordingly have as powerful an effect as a very much larger dose of a nitrite.

There is yet another possible reason why nitro-glycerine may act more energetically than a nitrite, and one which is capable to a certain extent of explaining the acuteness of its action as compared with that of a nitrite. As the nitro-glycerine, according to the theory advanced, requires to be decomposed before it begins to act, the nitrous acid formed is in a nascent state, and nascent bodies are well known to be much more active than the same bodies in their ordinary state. The nascent nitrous acid of nitro-glycerine exerts a more incisive action on the blood and tissues than the nitrous acid of a nitrite. This nascent powerfully oxidising nitrous acid will be set free wherever the nitro-glycerine comes in contact with alkaline material—thus, in almost every tissue and organ of the body, and perhaps more readily in these than in the blood; for in these the disintegrated alkaline tissue-waste is also in a nascent condition and possesses a greater decomposing action. So the vaso-motor and respiratory centres are quickly and energetically attacked.

If the difference between the action of nitro-glycerine and that of a nitrite be such as I have attempted to represent, it is clear that, were it not for the disagreeable and sometimes alarming symptoms which accompany the action of the former, nitro-glycerine would be a preferable remedy to a nitrite. For none of its effect is lost in the process of its absorption. Indeed, in certain cases, it is highly probable that it will act where a nitrite fails to act, as, for example, in cases of gastric derangement where the stomach is distended and constantly filled with a large quantity of a strongly acid fluid, and the mucous membrane is thickened and possesses a weak absorptive power.

In view of the continued, and perhaps extended, use of nitro-glycerine in therapeutics, it may be desirable to add a few observations which I have made on its physical characters, more precise than those to be met with in previous publications.

Nitro-glycerine is perfectly colourless, and not of a clear yellow colour as is stated in most of the papers on the chemistry of this body. The colour is due to the imperfect removal of the acid, or to the use of soda which is commonly used for washing it, and which decomposes it with the production of a reddish-brown colour. It has no odour when cold, but has a sharp pungent odour when heated. Its taste is sweet, and not unlike that of glycerine, but is more pungent. As regards its solubility:—one gramme dissolves in about 800 cubic centimetres of water; with difficulty in 3 cc. of absolute alcohol, easily in 4 cc.; in 10·5 cc. of rectified spirit (sp. gr. 0·846); in 1 cc. of methylic alcohol (sp. gr. 0·814); in 4 cc. of methylated spirit (sp. gr. 0·830); in 18 cc. of amylic alcohol; in every proportion in ether; so also in chloroform, in glacial acetic acid, and in carbolic acid; in less that 1 cc. of benzol; in 120 cc. of carbon bisulphide; and to a very limited extent, if at all, in glycerine. Its solutions in water and in alcohol I have kept for nearly four months without their exhibiting the slightest evidence of decomposition; and I have no reason for believing that they will not remain undecomposed for a much longer time.

ATHETOSIS.

WITH REMARKS BY W. AINSLIE HOLLIS, M.D.

HARRY P., age 14, no occupation, came to the Sussex County Hospital as an out-patient on 8th January last. He was not a strong child. About seven years ago he came home with chills and was in bed for some weeks: He had two fits at that time: and was subsequently found to have lost the use of his left limbs. A condition of "mobile spasm" affecting the palsied limbs commenced soon afterwards. The arm was first and mostly affected. No family history of chorea or other neuroses.

Present condition.—Fairly nourished, intellect tolerably good, perhaps somewhat below the average. The left legs are evidently shorter than the right.

Left fore-arm from olecranon to radio-carpal joint,			10 inches.
Right ditto,	ditto	ditto	9 inches.
Left thigh,	. . .	. . .	1 ft. 3 inches.
Right ditto,	. . .	. . .	1 ft. $3\frac{1}{2}$ inches.
Left leg from middle of patella to sole of foot,			1 ft. 7 inches.
Right ditto,	ditto	ditto	1 ft. $7\frac{1}{2}$ inches.

The muscles of the left side of the face, especially those at the corner of the mouth (*e.g.* orbicularis, zygomatics, levator anguli oris, &c.), the muscles of the left hand and fore-arm, (especially the flexors and extensors of the fingers, the pronators and supinators of the hand), and the muscles of the tarsus, successively undergo slow involuntary rhythmic contractions. No muscular hypertrophy of the affected limbs is observable,

although as the circumferences of the two fore-arms at their thickest parts are equal (viz. nine inches), there may be a comparative hypertrophy of the shorter left arm. The disordered muscles are not entirely removed from voluntary control: yet their inco-ordination is such that any volitional act with the left hand is attempted with difficulty, and is only performed after some assistance from the sound limb. The rhythmic movements are usually increased whenever a voluntary action is performed by the healthy members. He walks upon his left toes with the foot inverted, and the knee bent. The left arm is usually held on such occasions stiffly out at an angle to the body. The muscular spasm can be overcome by force. There is no pain attending the movements, and the cutaneous sensibility seems to be normal. Patellar reflex is stronger in the affected limb than in the other. The movements are believed to cease during sleep, and the disordered muscles are considered to be more tractable upon waking than at other times. Tongue is protruded naturally, speech not much affected. The hearing is good. There is total blindness of the left eye with atrophy of the optic nerve; amblyopia of the right eye. He was able to see well before his illness seven years ago. He sleeps well, and his general health is good.

The plantar reflex was somewhat exaggerated in both the lower limbs—mostly so on the diseased side. When his attention was diverted, tickling the palm of the affected hand produced a jerking reflex. If he was asked to prevent this muscular action whilst the tickling was continued in the open palm, the flexors slowly closed the hand without a jerking movement.

Remarks.—The above case is a fairly representative one of this disease. The name Athetosis is derived from *ἄθετος*, without fixed position, out of place—and Aristotle calls *μονάς*, a unit, *ἄθετος οὐσία*, a free molecule in distinction to a point or *θετός οὐσία*. It does not imply motion or movement of any kind, as Dr. Hammond, who introduced the term, supposes. The disease may be either congenital (as in Dr. Shaw's cases, (*St. Bar. Hosp. Reports*, vol. ix.), or secondary, as in the original cases cited by Dr. Hammond, those of Dr. Gairdner, others by more recent observers, and in the present case. Dr. Hammond says of the muscular movements attending this disease, that

they are regular, and to some extent under the control of the will. They continue during sleep (?). There is pain in the affected parts, and the sensibility of the skin is lessened, and a feeling of numbness prevails over the side of the body corresponding to the disordered motility.

The case I have the honour of bringing before your notice to-day differs in some important respects from either Hammond's or Gairdner's, as regards the cutaneous sensibility. It resembles some of the cases related by Shaw, as regards the affection of the facial muscles, and the loss and impairment of the eyesight corresponds to a case detailed in the *London Medical Record* (March, 1879), wherein amaurosis of both eyes preceded the athetic symptoms. In a case of Gairdner's there was also atrophy of optic disc.

M. Oulmont (*Étude Clinique sur l'Athétose,* 1878) considers that there are two forms of athetosis—the unilateral or hemiathetosis, and the bilateral or general athetosis. The disease frequently follows an attack of hemiplegia, and is closely allied to the spastic contracture observable in children after paralysis, but differs from it in the slowness and spontaneity of the movements. It is readily distinguished from the jerking movements of chorea or disseminated sclerosis. In athetosis the contraction of an affected muscle is gradual, "one part may be quite at rest, whilst the other is in motion." In chorea the whole muscle is contracted at once. When the speech is affected in athetosis it partakes of a drawling character, and cannot well be mistaken for the syllabic utterance of disseminated sclerosis.

The intellect is fairly good; Dr. Bernhardt (*Virchow's Arch.*, May, 1876) describes a very similar case to the one exhibited, the details of which I need not give.

I shall next briefly consider the pathology of this disorder. There are very few recorded autopsies upon patients dying of this disease, and consequently its pathology has not been fully investigated. Dr. Gowers (*Med. Chir. Tr.*, May, 1876) mentions a case of extreme inco-ordination of the arm after hemiplegia. The necropsy showed a cicatricial induration extending through the middle of the optic thalamus. There was no degeneration of the spinal cord.

Another case is given in the *Lancet* (March, 1879), by Dr. Murrell. Left athetosis, following infantile palsy, was found dependent on a diminished size of the right frontal and parietal lobes of brain. The right pyramid and corpus striatum were shrunken. The optic thalamus and spinal cord were not affected (*Virchow's Jahresb.*). In a third case (*Soc. Anat.*, 1878. *Lond. Med. Rec.*, March, 1879), a calculus about the size of a French bean was found in the lenticular nucleus of the left "corpus striatum," (printed "restiform body," evidently in error). The left cerebral peduncle was smaller than the right, but the lobe was unaltered in size. The patient had shown no signs of paralysis during life. The athetic movements were confined to the right hand, fore-arm, toes, and metatarsal joints. In Dr. Ringer's case (*Practitioner*, xxiii. p. 164), of athetosis with unilateral sweating (subsequent to hemiplegia and hemianæsthesia of right side, from which paralysis the patient recovered), there was a cyst in the posterior part of left corpus striatum implicating the lenticular nucleus, and the white fibres on the outside of the thalamus. The medulla and cord were apparently healthy. These are the only recorded instances of autopsies in this disease that I can obtain. In all of them the lesions were localised in the basal cerebral ganglia, and with the exception of Dr. Gowers's case, which I can scarcely consider a typical instance of the disease, the corpora striata were involved.

The essential phenomena of athetosis, namely the slow involuntary contraction of successive muscles, together with the somewhat increased patella reflex, will necessarily point to a want of inhibitory or controlling influence over the motor ganglia of the central nervous system. We here perceive muscles contracting automatically, and in H. P's. case, as in Dr. Murrell's above alluded to, we have evidences of some atrophic changes prior to the occurrence of the spastic contracture of the muscles. The shortening of a limb is, as we well know, a common sequence of infantile palsy, that is of granular degeneration of the grey anterior columns of the cord, and the multipolar ganglion cells therein.

Dr. Taylor exhibited a young woman suffering from athetosis before the Clinical Society of London (March, 1878). In this

case the muscular disturbance had evidently been preceded by atrophic changes in the affected left limbs, which were shorter than the right, although the muscles were hypertrophied. Dr. Taylor felt some doubt apparently as to the correct nomenclature of the tonic spasmodic disorder of his patient. I do not here wish to assume that all cases of athetosis are necessarily preceded by changes in the central motor tracts of the cerebrum or cord, but we know how frequently we have a history in this disease of an antecedent paralytic seizure, and how closely the motorial phenomena resemble other forms of muscular inco-ordination occurring after palsy.

It may happen that a lesion of the motory basal ganglia of the brain, the possible centres of co-ordinate muscular action, according to Prof. Burdon Sanderson—it may happen, I repeat, that such a lesion can diminish the inhibitory functions of the will over certain motor ganglia in the cord, and release them and their muscular connections from voluntary control. These ganglia, thereby rendered independent and irresponsible, will, like certain irresponsible people, be too prone to show a little transient authority at the expense of the general welfare. Muscular contractions originating in some slight and trumpery sensory stimuli, will be repeated, whilst the brain will be powerless to control the restive members.

If we watch Harry P. when he is performing any complicated muscular actions with his sound hand and arm, we shall perceive that there is a marked increase in the inco-ordinate movements of the athetic limb. A similarly exaggerated condition of disorderly muscular contraction affects the left muscles of the mouth and face when he speaks, dragging as it does the oval angle in various directions towards the affected side. It would seem that whenever a voluntary effort is made to execute certain movements by the sound muscles, the will acted in an indirect way, as a stimulus to the athetic muscles, and induced contractions in them also, but with this important difference, that whereas on the sound side the act of volition was precise and restricted to a few definite muscular movements, its influence upon the disordered muscles was inco-ordinate, indefinite, and resembling the so-called irradiation of nervous impressions, whereby a single stimulus may induce various dissociated sensory or motor phenomena.

In Harry P.'s case, this result is probably brought about by means of the commissural fibres either of the brain, or of the cord—of both possibly. These considerations, as also the fact that the motor phenomena cease frequently during sleep, induce me to offer as a provisional hypothesis, that the athetic movements, such as we here observe, partake essentially in their nature of reflex phenomena.

Reviews.

Books on Diseases of Women.

Practical Treatise on the Diseases of the Uterus, Ovaries, and Fallopian Tubes. By A. Courty. Translated by Agnes M'Laren, M.D. London: J. and A. Churchill. 1882.

Professor Courty's work on the diseases of the uterus has been for many years a leading one among French books on the subject: it has attained to a third edition, and it is from the last edition that the present translation has been made. The author has been fortunate in a translator, for Miss M'Laren was formerly his pupil and afterwards his private assistant; she was therefore peculiarly well-fitted to translate the book and place the Professor's views before English readers, and she has performed her task thoroughly well.

The work is divided into Two Parts and an Introduction. The Introduction consists of about ninety pages and treats of the anatomy, physiology, and teratology of the organs of generation; and with regard to it we need only say that the account of the subject is full and up to our present knowledge.

Part I. contains a general survey of uterine diseases. It treats of their symptoms, diagnosis, prognosis, frequency, and treatment; while Part II. treats of uterine diseases in detail. The first part really contains the principles which underlie the second part,—the principles which, according to the author, should guide us in the treatment of uterine disease. These are explained more fully, more in detail, in the second part, and their application under the various conditions met with in disease is given.

Professor Courty belongs to that school of gynæcologists to whom apparently trifling disorders of the uterus are of the greatest importance; we therefore find the remote and reflex symptoms of uterine disease abundant, ulcerations of the cervix and deviations of the uterus with their evil results elaborately described, and the methods of treatment numerous and varied. Of the doctrines of the school to which the author belongs the work is one of the ablest expositions, and it is one which deserves to be studied by every one engaged in the practice of gynæcology.

A Synoptical Guide to the Study of Obstetrics. By ROBERT BARNES, M.D. London: Smith, Elder, and Co. 1883.

THIS little work is, as its name indicates, a sort of elaborate table of contents of a complete work on midwifery, and is intended to aid lecturers on, as well as students of, the subject. The work is full, and is well adapted to the purpose for which it was written.

The Pathology, Diagnosis, and Treatment of the Diseases of Women. By GRAILY HEWITT, M.D. Lond., F.R.C.P., Professor of Midwifery and Diseases of Women, University College, and Obstetric Physician to the Hospital; formerly President of the Obstetrical Society of London, Honorary Fellow of the Obstetrical Society of Berlin, &c., &c. Fourth Edition; revised, enlarged, and in great part rewritten, with numerous illustrations; pp. 908. London: Longmans, Green, and Co. 1882.

THE special feature of this book is that it contains the most extended, complete, and elaborate exposition extant of what is known as the "mechanical system of uterine pathology." Dr. Hewitt's views on this subject have already been made known to the profession through the medium of scattered lectures, contributions to society transactions, journals, &c., but in this work his teaching is before us in a complete and systematic form. When the third edition appeared, he attached great importance to changes in the shape and position of the uterus. Now, he is still more convinced of their gravity, has formulated a theory to account for their production, and has laid down minute directions for their treatment.

The theory is this. One of the most frequent morbid conditions met with in women, one which "should be admitted into the list of recognized diseases," is "*chronic starvation*." That is, long-continued insufficiency of food, either as to quality or quantity, or both. This chronic starvation leads to malnutrition of the uterus, which consequently becomes abnormally soft. Being soft, it bends more readily than in health, and the pathological condition known as "*flexion*" is produced. By far the larger proportion of symptoms referred to the pelvic organs depend upon flexions or displacements. Our author, in his own practice, finds that they form about 70 per cent. of the cases that came under his notice. A flexion is often a "terrible misfortune." Flexion is a common cause of amenorrhœa, of deficient menstruation, and of excessive menstruation. It is the great cause of dysmenorrhœa and of sterility. Most cases of congestion and hypertrophy of the uterus depend upon flexion. The so-called "uterine fungosities" are, in the author's opinion,

generally nothing but the mucous membrane of the uterus swollen as a consequence of flexion. The vomiting of pregnancy depends upon flexion. Flexion leads to abortion, and after abortion to retention of placenta or membranes. Hysteria, convulsions, hystero-epilepsy, cephalalgia, mental disturbances, nausea, and vomiting are among the protean symptoms to which flexion may give rise. Of so common a complaint as leucorrhœa, flexion is the most frequent cause. Next, we learn that the treatment of flexion requires to be careful and prolonged. Attention must be given to the general health, in order to overcome the state of malnutrition which is the first link in the chain of evil. But if this treatment is not combined with correction of the shape of the uterus, its only effect will be to harden the uterus in its vicious shape, and so stereotype the mischief. Therefore diligent treatment must be used to remove the flexion. The sound must be passed twice or three times a week, and by it the uterus moved into the proper position and retained there for a few minutes. The patient must lie for weeks or months in the position best calculated to throw the uterus into the position the opposite of the abnormal one it has unfortunately assumed. And unless these means are sufficient, which only can be counted on in the milder cases, an appropriate pessary, either vaginal or inter-uterine, must be worn.

The importance of Dr. Hewitt's views can hardly be overestimated. The conditions on which he lays so much stress are very common. If, as some eminent gynæcologists hold, flexion of the uterus is a condition which seldom or never causes disturbance of health, then treatment of the kind we have outlined,—the frequent use of sound and pessary, the prolonged confinement to the couch, etc.,—is exceedingly hurtful, both morally and physically. But if Dr. Hewitt be right, his treatment, in spite of the objections to it, is absolutely necessary.

It is impossible, in the space at our disposal, to attempt a critical estimate, or even a summary, of the arguments by which Dr. Hewitt supports his position. But we think that even those who most thoroughly oppose his teaching will admit that his opinions are set forth in a calm and dispassionate manner, with courtesy to opponents and with full knowledge of what is to be said for and against his views; and that it is the author's evident desire to discover and to teach that which is true.

Clinic of the Month.

Homœopathic Treatment of Insanity.—Dr. S. H. Talcott, having tried the homœopathic treatment of insanity, cured, in seven years, 45 out of 1,100 patients. This rate of recovery, unless it is misprinted, is ludicrously small. At the Camden County Asylum, also, a homœopathic superintendent tried the so-called homœopathic method, and after six months stated publicly that the experiment had been a failure. (*American Journal of Nervous and Mental Diseases*, Dec. 1881.)

The Uses of Creasote.—Hager finds that consumptives and sufferers from chronic catarrh improve under its use, but only when true creasote, and not carbolic acid, is employed. It increases the general nutrition of the patient. Its anti-asthmatic action is also well marked. The dose should be as small as possible, but often repeated. The proper doses for an adult are 2 to 4 centigrammes two or three times daily. The maximum single dose is 5 centigrammes [$\frac{3}{4}$ grain], and the maximum for the day 2 decigrammes. The *pilulæ kreasotatæ* contain 0·0167 grammes [$\frac{1}{4}$ grain] in each pill. Consumptives in the last stage may take two or three of these pills two or three times a day, according to bodily size, and, if they feel tolerably well after some days' use, they should take two or three pills uninterruptedly night and morning, dropping them, perhaps, one or two days a week. Pure creasote is pale yellowish, exudes an oily liquid, and is of specific gravity 1·06. The specific gravity should be fixed at 1·050 to 1·080, as the lighter creasotes are either impure or not properly creasotes at all. Creasote is best administered in pills. The pilular mass is made by melting together two parts of yellow wax and one of creasote, to which other suitable ingredients are added in form of powder, as quinine, salts, salicylic acid, gentian root, etc. Any addition of ether or spirits of wine, to give consistency to the mass, should be avoided, and is not necessary. (*The Analyst*, No. 80, vol. 7.)

Disease of the Coronary Arteries and Chronic Myo-carditis.—Cases not unfrequently occur, especially

among well-to-do people, more rarely in the poorer class of persons over middle age, in which death occurs suddenly, without previous symptoms, excepting, perhaps, a feeling of constriction or pain at the chest. Dr. Carl Huber gives a number of such cases in which the cause of death appeared to be sclerosis of the coronary arteries, and consequent chronic myocarditis. The consequences of this myo-carditis are aneurism of the heart, thrombosis, dilatation, and hypertrophy. The clinical symptoms are angina pectoris, steno-cardia, and asthma. These symptoms generally occurred in paroxysms some months before death, generally after excitement, either bodily or mental, several times after dinners. In some there was irregularity or intermittence of the pulse, occasionally cardiac bruits; sometimes there was a sudden giddiness with temporary loss of consciousness on stooping, walking quickly, or going up stairs. The attacks were sometimes also accompanied by symptoms of collapse. Death sometimes occurred almost at once, but at other times several minutes, hours, or even days elapsed, during which time there were the symptoms of cerebral apoplexy, paralytic conditions, and alterations in the cardiac rhythm. The peculiarity of this cardiac affection is that it has nothing whatever to do with endocardial or pericardial disease, but depends on arterial sclerosis. Cardiac apoplexy, the author considers, is a condition to be re-instated in its old place as a well-marked disease, like cerebral apoplexy. It sometimes occurs in young individuals, the general cause of such occurrence being alcoholism or syphilis. (*Virchow's Archiv*, vol. 89.)

Influences of Disease on the Size of the Heart.— This subject has been investigated by Dr. Spetz. He finds that in typhus there is no characteristic change in the dimensions of the heart and the large vessels; the same is the case in puerperal pyæmia. In phthisis the heart is diminished, and especially the left ventricle. The right ventricle is often somewhat diminished, but not in proportion to the diminution in the weight of the body. It is sometimes even hypertrophied, but not as a rule. The ratio between the depth of the left ventricle and the circumference of the aorta is diminished, and as this is not compensated for by hypertrophy of the muscular walls of the ventricle, there is a diminution in the arterial tension. Consequently the pulse in phthisis is soft and small. In cancer the depth of the left ventricle is still more diminished than in phthisis, and the right ventricle is affected almost as much as the left. In granular kidney, both ventricles increase very much, but especially the left. The aorta is not correspondingly dilated. In consequence of this the tension in the arteries is very greatly increased. In myo-carditis, also, the heart is dilated

and hypertrophied, but the left and right ventricle are almost equally affected. In chronic emphysema both ventricles are much dilated, with very little thickening of the muscular walls. Both ventricles are nearly equally affected. The pulse is full, but small and languid. (*Deut. Archiv für klin. Med.* p. 138, vol. xxx.)

Calcification of the Pericardium.—M. Gustave Rivet describes a case of ossification and calcification of the pericardium in a woman of seventy-eight years of age, who was admitted for general dropsy, and, in the absence of cardiac symptoms, was supposed to be suffering from renal disease. She died in three days, and post mortem the heart was found in the following condition. It was surrounded almost over its whole surface by a cuirass or carapace, under which the two layers of the pericardium were adherent. The cuirass extended from the base towards the apex, stopping at the auriculo-ventricular furrow, except for one or two outshoots on to the auricle; and reaching to about one inch from the apex. Transversely, it involved the whole posterior and the greater part of the anterior surface. The part not involved measured 2 to $2\frac{1}{2}$ inches; and here the two layers of the pericardium were not adherent. The thickness was greatest over the base of the right ventricle, becoming here 0·4 of an inch; and over the left ventricle 0·2 to 0·3 of an inch. Beyond slight atheroma of the aorta the other vessels were healthy. (*Progrès Médical*, Dec. 23, 1882.)

Milk-Diet in Graves' Disease.—Dr. Schnaubert, in Botkin's *Ejenedeln. Klin. Gaz.*, 1882, No. 13, speaks very favourably of the value of exclusive milk-diet in cases of exophthalmic goitre. In one of his patients, three weeks' treatment by milk restored digestion and general health, and so greatly improved all symptoms, that some weeks later the patient left the hospital relatively sound and remained so nearly two years. At this time, she returned with highly developed signs of the disease, and though the milk treatment again greatly relieved the patient's condition, she soon died. The necropsy showed hyperplasia in the cervical sympathetic ganglia, pigmentation of roots of the cervical nerves, and cerebro-spinal hyperæmia. The milk-treatment proved as beneficial in two cases of Graves' disease, reported lately by Dr. Catharina Shumova in Nos. 1-5 (1882) of the same journal. (*Med. Record*, Feb. 1883.)

Ulceration of Arteries in Contact with Pus.—M. Théophile Anger mentions some cases attended with violent hæmorrhage, where the blood came from the neighbourhood of large cavities containing pus. Three of these occurred in the

popliteal space. In the first the diagnosis was rendered complete by a post-mortem examination, which revealed a large rent in the popliteal artery a centimetre in length. The abscess which preceded the hæmorrhage was due to an attack of acute osteomyelitis in the lower end of the femur. The second case was due to the same cause, but was not confirmed by an autopsy. The third case, also occurring in the popliteal space, simulated at first acute ostitis and periostitis of the femur, but appears to have undergone complete cure. In the last case the abscess occurred in the neighbourhood of the lesser trochanter in a child who was affected with tuberculosis, in addition to acute suppuration round the hip-joint of long standing. Death was caused by ulceration of the femoral artery. In this latter case the suggestion is hazarded that the ulceration was preceded by and due to the deposition of tubercle in the arterial walls. In the discussion following the reading of the above cases several others of a similar nature were recorded. (*Bulletins et Mémoires de la Société de Chirurgie de Paris,* Nov. 1882.)

A case of ulceration of an artery within the cavity of a cold tuberculous abscess is also recorded by Charcot in the same publication for Jan. 1883.

Subperiosteal Resections.—An interesting record of the results of this operation is afforded in the following case. The operation was undertaken in April, 1871, in consequence of a bullet fracture of the humerus. Two years later the patient was able to return to his work as a stone mason, and the arm presented in every respect a healthy appearance, with the exception of a paralysis of some of the radial muscles. All went well, and the patient remained at work till July, 1882, when some inflammation appeared in the arm, and in September it was amputated at the shoulder joint, in consequence of severe inflammation. The examination of the arm confirmed the diagnosis of ostitis in the new bone. The patient made a good recovery. (*Bulletins et Mémoires de la Société de Chirurgie de Paris,* Dec. 1882.)

Induration of the Corpus Cavernosum of the Penis in connection with Glycosuria.—Four or five cases of this affection are quoted by Mons. Verneuil. He draws attention to the more ordinarily recognised causes of this affection, viz. syphilis and gout, and explains how useless all treatment has proved in his hands. The symptoms appear to be those of hardness occurring along the corpus cavernosum and limiting erection, associated with glycosuria. The latter may be intermittent in character. (*Ibid.,* Jan. 1883.)

Treatment of Puerperal Eclampsia.—Six cases of puerperal eclampsia, treated by diaphoresis and with but one

fatality, have been reported by Dr. Karl Breus. The patient is immersed up to her neck in a bath at a temperature of 100·4° F., and the water is then further heated to as high a degree as bearable. The patient's stay in the bath should be about half an hour, or until a copious diaphoresis commences on the face and head. She is then packed in a sheet, and several warm, thick blankets wound round her; the face being alone uncovered, to prevent any impediment to respiration. These may be removed in three or four hours. Thirst may be relieved by moderate use of soda-water. On the reappearance of coma or convulsions, the bath and pack may be again resorted to. Dr. Breus believes hydrate of chloral given in enemata to be a valuable adjunct to the bath. In the first two reported cases the convulsions occurred before delivery—in the first at the ninth and in the second at the sixth month. The baths were resorted to with the result that the convulsions ceased, both patients being delivered of living children. Convulsions occurred both during labour and after delivery in the third case, and towards the end of labour in the fourth case; but free use of the baths resulted in the entire recovery of both patients. The fifth case presented an untoward complication, in the shape of cirrhosis of the liver. The patient was comatose when admitted into the hospital, and died on the third day. In the sixth case the convulsions did not appear until the twenty-third day after delivery, but the patient made a good recovery. (*London Med. Record,* March, 1883.)

The Connexion between Diseases of the Abdomen and of the Right Heart.—Dr. Passerini, in a short but weighty article (*Gazz. degli Ospitali,* Jan. 3), gives his views on this subject. Potain in 1878 was the first to call attention to the fact that affections of the digestive apparatus may give rise to disease of the right side of the heart. With the exception of Tessier and Frank, who wrote in 1879 and 1880 respectively, no other author has written on this relationship. The author relates three cases of tricuspid insufficiency due indirectly to peritoneal effusion. Auscultation revealed at the tricuspid orifice a prolonged first sound, and a regurgitant murmur. The second sound was accentuated, more especially over the pulmonary orifice. When the fluid in the peritoneum was removed, there was marked improvement in the character of the heart-sounds. The mode in which the derangement of the heart is effected is regarded as purely mechanical. Owing to the compression, there is in the abdomen a venous ischæmia, whereby in the thorax there is induced a venous hyperæmia. The right side of the heart thus becomes engorged. Moreover, owing to the pressure from below, the diaphragm becomes fixed; the lungs cannot expand freely and express their contained blood.

The consequence is that the flow of blood from the pulmonary arteries through the lungs is obstructed. Thus the right heart is exposed to a twofold strain; the greater pressure of the incoming blood from the thoracic venous hyperæmia; and the obstruction to the outgoing blood from the inefficient expansion of the lungs. In confirmation of these views, the author brings forward other facts. He quotes the observation of Larcher (1859) and of Depaul (1880), frequently verified by himself, that in advanced pregnancy the first sound over the pulmonary orifice becomes accentuated, and that sometimes the first sound over the base of the xiphoid cartilage becomes prolonged and blowing. The same phenomena are observed in cases of ovarian cysts and of other large abdominal tumours. Moreover, it is possible in perfectly healthy persons to induce a well-marked accentuation of the sound at the pulmonary orifice by compressing the abdomen, or even by simply causing the subject to hold his breath. From a practical point of view, it would often be of the greatest importance to know whether the abdominal affection was the cause of the heart-mischief. (*London Med. Rec.*, March, 1883.)

Cardio-pulmonary Symptoms consequent upon Gastro-hepatic Disorders.—Dr. Barié sums up his researches as follows:—

1. Certain dyspeptic conditions of stomachic, intestinal, or biliary origin react on the cardio-pulmonary apparatus, giving rise to morbid clinical phenomena, which may be grouped into four classes: *a.* Cardiac symptoms only, as palpitation or intermittency. *b.* Both pulmonary and cardiac symptoms. As regards the former, there is more or less powerful oppression, extending even to orthopnœa, with impending suffocation, which supervenes almost immediately after food and ceases when digestion is accomplished—to reappear, however, after the next meal. As regards the latter, there is right cardiac dilatation, entailing sometimes secondary tricuspid insufficiency, with its clinical consequences. Physical examination reveals a galloping rhythm of the right ventricle and marked accentuation of the pulmonary second sound, due to increased arterial tension in the pulmonary circulation. During the attack of dyspnœa, moreover, the pulse is small, soft, and compressible; there are cyanosis and coldness of the face and extremities, dilatation of the pupil, and at times some hæmoptysis. *c.* Symptoms akin to angina pectoris. *d.* In the last class, the symptoms are less marked. There is some slight panting after meals, with accentuation of the pulmonary second sound, but without discoverable cardiac dilatation. These forms may occur separately, or run more or less into one another.

2. These phenomena result from reflex irritation, which, starting from the digestive tract (stomach, intestine, or biliary canals), is reflected to the lungs, whose capillaries are kept in a state of spasmodic contraction; the tension is suddenly increased throughout the pulmonary system, and the right ventricle, compelled to struggle against this obstacle, at first becomes dilated and then hypertrophied.

3. Experimental physiology would appear to demonstrate that these reflex phenomena are entirely under the control of the sympathetic system; but clinical experience nevertheless shows that the pneumogastric has at least some share in the centripetal transmission of impulses from the stomach or biliary passages to the reflex centre.

4. Cardio-pulmonary phenomena are observed only as consecutive upon the lighter affections of the digestive tract (catarrh of the stomach or bile-ducts, calculi, &c.); they are not met with in affections which disorganise the tissues, as diffused chronic inflammation or organic degeneration.

5. The phenomena principally occur in nervous and highly impressionable people, and in women rather than in men. The predisposing causes are neurotic, chlorotic, and hysterical temperaments.

6. The dyspeptic conditions principally associated with cardio-pulmonary troubles are—in the stomach, simple catarrh, and the dyspepsia consequent upon tuberculous, renal, uterine, or cardiac affections; in the liver, catarrhal jaundice, biliary calculi, &c.

7. Generally the prognosis is not grave, but those whose dietetic regimen is bad are subject to recurrent attacks. When, however, the exciting cause (arrest of a calculus in the common bile-duct, for instance) persists long enough to give rise to extreme dilatation of the right ventricle, tricuspid insufficiency may result, whence the patient may succumb to cardiac syncope.

8. A milk-diet is the only efficient remedy. Its effect in cases of gastric origin is marvellous; though in those consequent upon hepatic trouble, its action is somewhat slight and uncertain. (*Revue de Médicine,* Feb. 1883; *Lond. Med. Record,* April 1883.)

Dr. Germain Sée (*Boston Med. and Surg. Jour.*, No. 1, 1883), in the course of a clinical lecture on overwork and strain of the heart, describes dilatation of the heart without valvular disease as a result of jaundice, dyspepsia, enteritis, diarrhœa, &c.

Ferrocyanide Pellets in Testing for Albumen.—Dr. Pavy recently communicated to the Clinical Society of London the following original method of testing for albumen in urine. The test-substance consists of a pellet composed of sodic ferrocyanide

and citric acid. This is placed, crushed or not, in a test-tube and the urine poured on it to the height of an inch. On solution of the test-agent in the urine a cloudiness or precipitate is subsequently formed, which indicates the presence and, roughly, the amount of albumen present. Heat is unnecessary (except to clear the urine, if lithates be present), and therefore a phial or wine-glass will do as well as a test-tube in many cases. Acid (citric) enough exists in the pellet to free the ferrocyanic acid by which it acts, and also to make the urine itself acid if alkaline, and so procure solution of phosphates and remove this source of fallacy. Oleo-resinous matter, if present, gives a turbidity; but this is so with the nitric acid test, and may be obviated by the same means as when using the latter. If preferred, the pellet may be dissolved in water, and the solution when slowly poured along a test-tube containing urine will give the characteristic turbidity at the junction of the two fluids if albumen be present. The ferrocyanide test is very delicate, and will detect the smallest quantity of albumen. The pellets are to be had of Mr. Cooper, 58, Oxford Street, London. (*British Med. Journal*, Feb. 17, 1883.)

Presystolic Murmurs, Organic and Inorganic.— Dr. A. E. Sansom, in his Lettsomian lecture on mitral stenosis, makes some interesting observations on organic presystolic murmurs. He describes the two principal forms of contraction,— the commoner button-hole variety, in which the adherent valve segments viewed from above form a flat, hymen-like surface, presenting a narrow transverse chink at the centre; and the much rarer "funnel mitral," a hollow cone with a round orifice at the summit projecting into the ventricle. He holds that the murmur is not necessarily associated with the final auricular contraction, but though intensified by this, may be and often is produced by the mere force of friction of the inflowing blood during *diastole* after being pent up in the resilient left auricle and pulmonary veins. In support of this view he relates a case in which a presystolic murmur from stenosis co-existed with a left auricle lined with clot, so as to render contraction of its wall impossible. He also urges as another proof the fact that the murmur of the mitral stenosis is often more strictly diastolic than presystolic. In such cases it is very difficult to diagnose from diastolic aortic murmur, especially when the latter is best heard at the apex of the heart. Another possible source of fallacy is friction from pericarditis over the auricles. (*Lancet*, Feb. 24, 1883.)

Dr. Austin Flint quotes cases to show that a presystolic mitral murmur may occur with a healthy mitral valve. In these cases the left ventricle was dilated and hypertrophied; the mitral valve, post mortem, normal and competent, and the coronary arteries narrowed. A systolic murmur existed along with the

presystolic. He explains the latter thus: The curtains of the mitral valve are pushed up and apposed, though not firmly, by the blood flowing in from the left auricle during ventricular diastole. Then follows auricular contraction, causing a jet of blood to fall on the central point of the apposed curtains, producing vibration of these and consequent presystolic murmur. He compares this to the vibration of the lips when a current of air is sent through them. He considers gradual disappearance of such a murmur a bad omen, probably indicative of loss of power in the auricular wall. [*Pract.* xxix. 131] (*Lancet*, Jan. 27, 1883.)

Pus and Microzymes.—The formation of pus has been shown by many observers to be closely connected with the presence of microzymes. Uskoff published some experiments in *Virchow's Archiv*, volume 86, in which he stated that small quantities of indifferent fluids, such as water, milk, and olive oil, produced no pus, but that large quantities did so. These experiments have been repeated by Orthmann, and he finds that when sufficient antiseptic precautions are taken, even very large quantities may be injected without any pus-forming. (*Virchow's Archiv*, vol. xc. page 549.)

Nervous Affections following Mumps.—Apart from the clinical interest of the following case as an instance of a very grave sequela of what is looked on as no very serious ailment, there is that arising from the illustration it affords of the "directing sense" of the semicircular canals; indeed the staggering gait would be unaccountable but for physiological experiment. The author, Dr. S. Moos, of Heidelberg, saw the case—a boy, aged thirteen—only once, about three weeks from the commencement of the illness. It was stated that hearing was totally lost on the fifth day, vomiting set in on the sixth, and staggering gait was observed on the eighth day, which, however, gradually passed off. There had been no unconsciousness. Deafness was complete, except by bone-conduction for low tones in the right ear. The right membrane was more opaque than normal, and not so concave as the left. These trifling local signs of ear-disease lead the author to suggest that the double labyrinthine affection may not have arisen directly from the parotids, but may have been metastatic, like the orchitis following some cases of mumps. There was clearly no meningeal inflammation, nor any trace of direct communication from the parotids. There would be, no doubt, some interference with the venous circulation in the labyrinth. The author states that some fibres of the right cochlea most probably escaped; to which might be added, perhaps, the semicircular canals, which could only have been temporarily congested or anæsthetic, as it is

unlikely the central organ of co-ordinating muscular movement could have formed new peripheral connexions so rapidly.

Dr. G. Brunner, of Zurich, brings forward a case which occurred in his practice in 1871 of unilateral deafness following mumps. The patient, a female aged thirty, perceived noises in the right ear like running water, and dizziness, "soon after" the swelling of the parotids. The evening of the same day she became deaf on the right side. The swelling of the parotids went down in eight or ten days; the deafness and noises in the ear were permanent; the vertigo soon passed off. The membrana tympani appeared normal. The state of bone-conduction is not specified, but appears to have been quite obliterated, as there is the remark that the galvanic effect, humming of flies, on application of cathode to right ear, was heard normally. The author sums up the characters of the five cases of which he has details as follows:—(1) The deafness is complete and incurable; (2) It is more frequently one-sided than double; (3) It develops early in the illness with subjective noises and dizziness, the last early passing off; (4) Pain only occurred in one case; (5) There is no noticeable fever; (6) Consciousness is not lost; (7) It occurs alike in children and adults. He contrasts the ailment with Menière's and Valtolini's diseases, with which it has so much in common; pointing out that the last appears confined to children, is double, is distinctly inflammatory, and is accompanied with head symptoms. The author does not consider the disease to be propagated from the parotid region, but metastatic—analogous to the orchitis referred to above, which is a rapid and early serous infiltration, without tendency to suppuration, usually one-sided, and sometimes leading to atrophy. [*Practitioner*, xxx. 54.] (*Archives of Otology*, March and June, 1882.)

Dr. Haslon relates another instance that occurred in a woman aged twenty-three, who, on the day after the mumps commenced, became completely deaf in the ear of the same side, there being neither discharge nor pain present. At the end of three years there is still constant tinnitus, and the watch is not heard on contact. There is a very faint perception of the tuning-fork held on the mastoid, increased by closing the meatus. The external ear and membrane tympani are quite healthy. As there have been no indications of cerebral complications in any of the reported cases, he thinks the labyrinth is probably the seat of the disease, and the suddenness and completeness of the deafness point to rapid and copious serous effusion. (*Philadelphia Med. News*, March 24, 1883.)

Dilatation of the Neck of the Uterus.—M. Chassagny, of Lyons, in a communication made to the Paris Academy of

Medicine, describes his method of thoroughly plugging the vagina, and producing rapid dilatation of the neck of the uterus. He places in the vagina a bladder, with which an india-rubber tube is connected; he then with the help of a siphon conveys into it the water contained in a receptacle placed about two feet and a half higher than the pelvis of the patient. The bladder becomes distended by the water, and soon fills the vaginal cavity. This brings on abundant secretion, and induces energetic contractions, resulting in the physiological dilatation of the os uteri, which is quickly completed by the mechanical action of the bladder. When the bladder is placed in the vagina the occlusion of the vulva is obtained by means of an apparatus which M. Chassagny calls the *Elytroptérygoïde.* It consists of a cylindrical speculum, which holds the bladder; this is forced out as the water enters, and the act of distension separates the valves of the speculum, which, resting on the sides of the pelvis, prevent the expulsion of the apparatus and of the bladder. M. Chassagny mentions, in his pamphlet, several instances of induced premature labour in cases of contracted pelvis, obstinate vomiting, eclampsia, etc. M. Chassagny describes two cases of vicious insertion of the placenta. In both cases he induced labour before the natural period by having recourse to rapid dilatation. There was not the slightest hæmorrhage, and two living infants were born. In another case, where the mother was in the last stage of suffocative catarrh, M. Chassagny effected in half an hour the safe delivery of a living child. The mother rallied for a few moments only. In post-partum hæmorrhage the bladder, by completely filling the uterine cavity, closes the openings of the vessels, and, by artificially restoring the pregnant state, determines uterine contraction. The water in the bladder slowly flows away, until the uterus is thoroughly contracted. (*British Med. Journal,* March 3, 1883.)

Hypodermic Injection of Ergotine in Prolapsus Ani.—In a thesis by Dr. Jette he describes what he has observed of this treatment, first introduced at the St. Louis Hospital by M. Vidal. The preparation is composed of one gramme of Bonjean's ergotine and five grammes of cherry-laurel water. It should be freshly made, so as to be of a limpid colour and fresh and agreeable odour. The injections are made every other day, commencing with fifteen drops, and, if this is insufficient, going on to twenty or twenty-five. This amount is inserted very slowly into the fibres of the sphincter to the depth of from two to four centimetres. Severe lancinating pain is caused, which then becomes dull and continuous, only ceasing after some hours. After three or four injections contractions of

the sphincter and fibres of the intestine commence. A sense of constriction and traction upwards is felt by the patient and observed by the surgeon. Various disturbances may take place; thus, if the doses of ergotine are too weak, there may be frequent desires to go to stool and to pass urine; and when they are too strong there may be spasm of the neck of the bladder, with temporary retention of urine. In some persons M. Vidal observed a tendency to vertigo or syncope, and painful præcordial constriction, with a hard, close, somewhat slow pulse. The latter injections become more active, as if cumulative, and cause a more prolonged tenesmus than the early ones. The duration of treatment varies from a few days to some weeks, and seems quite independent of the solution employed, the dose injected, and the intervals observed. To render the cure certain when once effected, it is of importance that three or four supplementary injections should be made. (*Revue de Thérapeutique*, March 4, 1883.)

Salicylates and Hæmorrhage in Typhoid Fever.—Mr. James Fergusson, in the *Brit. Med. Jour.*, Feb. 1883, p. 296, records some facts regarding the use of salicylate of soda in typhoid fever. Some cases were being treated with this drug, when it was noticed that there was an increase in the number of cases of hæmorrhage from the bowels; the salicylate of soda was supposed to be the cause of it. A foreign observer about this time noticed that salicylate of bismuth caused intestinal and nasal hæmorrhage. Dr. McLean Wilson, who succeeded Mr. Fergusson at the Perth Infirmary, confirmed these observations whilst treating typhoid fever with the salicylate. Dr. H. Tomkins, on the other hand, has published several cases in which he used salicylate of soda in large doses, without finding any trouble from hæmorrhage. His experience is that intestinal irritation is much lessened by the drug. Perhaps the greater or less purity of the salicylate used may have something to do with the difference in the results. [*Practitioner*, xxx. 365.] (*Lond. Med. Record*, April 1883.)

Extracts from British and Foreign Journals.

Cannabis Indica as a Cause of Insanity.—Dr. Davidson, of the Cheshire County Asylum, states from personal observation that in Turkey the term *hashish* is applied only to the poppy, *Papaver somniferum.* Cannabis indica is known by the name of *esrar*, an Anatolian word meaning *secret preparation.* In Morocco the same drug is know as *kif*, an Arabic word signifying *quietude* or *rest.* Many Moors, Berbers, and Arabs use the drug, sometimes by smoking it as a mixture with tobacco, sometimes by eating it in the form of a sweetmeat called *madjun*, made with butter, honey, nutmeg, and cloves. The aromatic *bahart* is used to cover the taste and odour of the drug, and to give it an aphrodisiac action. This combination modifies the direction of the delirious ideas, so as to produce in the ecstatic a series of most sensual and voluptuous visions. The state of exaltation is followed by intense melancholia, "while a poignant aspect of remorse and regret is depicted in every countenance." In Morocco, the *Santos* or saints, whom one is always meeting, and who are treated with great respect, are often confirmed consumers of the drug. They are "very frequently most dangerously homicidal, while not a few of them have been reduced to a state approaching that of drivelling imbeciles." Dr. Davidson also comments on the great prevalence of constitutional syphilis in Morocco, and its influence in causing insanity. He promises to publish his observations in a more extended form. It would be interesting to know whether the great persistence of constitutional syphilis is in any way contributed to by the habitual and widespread use of narcotics There is reason to think that the elimination of a virus like that of syphilis may be retarded in the case of a habitually narcotised system. Dr. Davidson's views on this subject should be of great interest. (*Journal of Mental Science*, Jan. 1883.)

Iodide of Potassium in Psoriasis.—Greve states that psoriasis is always curable by large doses of iodide of potassium.

He begins by small doses until the remedy is tolerated, and gradually increases the dose until he gives as much as thirty to forty-five grains. The curative effects are then evident. (*Tidsskrift for praktisk Med.*, No. 16, 1881).

Effects of Diminished Oxygen in the Inspired Air.—This subject has been investigated by Dr. Kempner, who finds that even a moderate diminution in the oxygen of the inspired air lessens the absorption of oxygen by warm-blooded animals very considerably. This occurs invariably in mammals; it is not so regular in birds which apparently have a greater power of compensating for diminution in the oxygen of the air by alterations in their rate of breathing. (This is interesting from a Darwinian point of view, inasmuch as birds in high flight have to breathe rarefied air.) In mammals usually the breathing is accelerated; in birds it is never accelerated, it is sometimes slowed, but is always very much deepened. (*Virchow's Archiv*, p. 290, vol. lxxxix.)

Action of Drugs on the Secretion of Milk.—A number of experiments have been made on this subject by Stumpf, who comes to the following conclusions:—

(1) *Alterations in the quantity of milk*:—Iodide of potassium diminishes it considerably; alcohol, morphia, and lead do not alter it; salicylic acid appears slightly to increase it; but pilocarpin does not.

(2) *Alterations in the quality of milk*:—Iodide of potassium causes disturbance of the glandular functions, and thus produces irregularity in the qualitative relation of the constituents of milk; alcohol and alcoholic liquors only increase the relative proportion of fat, and are to be discarded as dietetic agents for promoting secretion of milk; lead, opium, and pilocarpin do not alter the quality of the milk; salicylic acid appears slightly to increase the quantity of sugar.

(3) *Passing of drugs into the milk*:—Iodine passes quickly into milk, and in man disappears as soon as its administration is stopped; but it continues for a long time in the milk of herbivora. The quantity of the iodine which appears in milk is not a definite fraction of the amount administered, but varies considerably—these variations being due to differences in the individual. Iodised milk therefore cannot be used in therapeutics. Iodine in milk is not present in solution as iodide of potassium, but is combined with casein. Alcohol does not appear in the milk of herbivora. Lead when administered in small quantities only appears in traces in the milk, and continues to appear for some time after the administration has been stopped. Salicylic acid passes only into the milk in small quantities, even when

large doses are given. Somewhat larger quantities appear in human milk than in the milk of herbivora. [*Practitioner*, xxvii. 161.] (*Deut. Archiv für klin. Med.* p. 201, vol. xxx.)

The Action of Heating and Cooling upon Warm-blooded Animals.—In a paper published by Rosenthal in 1872 he observed that healthy animals preserved their normal temperature when exposed to cold, the vessels of the skin contracting and preventing loss of heat from the surface. When they were kept for a good while, however, in a warm place, the vessels of the skin lost their contractility and allowed the blood to circulate freely over the surface of the body, even when the body was exposed to cold. The blood thus became cooled down, and on its return to the internal organs gave rise, as he thought, to changes in them. He thus explained the readiness with which people catch cold after coming out of crowded rooms; and the utility of a cold tub in the morning seemed to get a rational explanation by its effect in training, as it were, the cutaneous vessels to contract readily on exposure.

A series of experiments has been made by Nasaroff on the effect of heating and cooling which have led to the following results:—When animals of the same species are heated or cooled artificially, the result is greatly affected by the age of the animal. Young animals have much less resisting power than old ones. The mere size has little effect. Well-nourished animals soon become accustomed to repeated heating or cooling, and retain their normal temperature much better, despite the changes in their surroundings. The contrary is the case in starved animals, and a much slighter extent of chill is sufficient to cause their death. When animals are repeatedly warmed and immediately afterwards cooled, their power of resisting heat becomes increased, but not of resisting the cold. On the contrary, such animals not only died in consequence of cold much quicker than animals which had not been previously warmed or cooled, but they died without their temperature falling much under the normal. The pathological alterations in the muscles, liver, and nerves of animals which have been either warmed or cooled, or both together, exhibit alterations both in the parenchyma and in the vessels; the latter are usually congested. These changes are the more marked, the greater the alteration which has been made in the animal's temperature, either in the direction of heating or cooling. When the internal organs are cooled directly by introducing a 0·7 per cent. solution of chloride of sodium into the abdominal cavity, the effects do not differ in any way from those caused by cooling the surface. The rapid cooling of the internal organs causes so little disturbance, that the author considers that

Rosenthal's theory is insufficient to explain the usual consequences of catching cold. (*Virchow's Archiv*, p. 482, vol. xc.)

Tears and their Secretion.—The secretion of the human lacrymal gland varies very much. Many conditions influence it, especially emotions (without considering weeping). In some experiments on this subject in which external influences were excluded as much as possible, Maynard found that the secretion occurred at a rate which would yield 6·4 grammes of tears in twenty-four hours. The prolonged action of atropine diminishes the secretion, eserine abolishes the action of atropine and quickly increases the secretion. Irritation of the cervical sympathetic gives no definite result. It appears rather to quicken the secretion, and sometimes the tears become turbid. Tears coagulate on boiling. They contain albumen and chlorides, but phosphates could not be detected. They contain 98·12 per cent. of water, 1·46 of organic matter, 0·41 of salts. (*Virchow's Archiv*, p. 258, vol. lxxxix.)

The Histogenesis of Carcinoma.—Dr. Carl Hempel Reed reports the results of some studies in the Pathological Laboratory of the University of Pennsylvania upon the histogenesis of carcinoma. He concludes that cancers have an exclusively epithelial origin, and summarises the essential points in favour of this view as follows. (1) Primary true cancers are found only in places where there is pre-existing epithelium. (2) No cancer has been proved beyond doubt to have originated heterotopically. (3) The cicatrisation of cancers explains the young connective-tissue infiltration. (4) Young connective-tissue cells or white blood-corpuscles are never seen inside the alveoli. (5) The epithelial cancer-cylinders are independent of the connective tissue. (6) The proliferating power of epithelium is normally greater than that of any other tissue. (7) Experiments show that the epithelial covering in the healing of ulcers is exclusively derived from the epithelium of the border, a most conclusive proof of this being the gradual advancement of the skin pigment from the borders of the healing ulcer; this fact, by analogy, forming a strong testimony in favour of the epithelial origin of cancer. (8) The transformation of connective-tissue cells into epithelial cells, in extra-uterine life, does not occur either physiologically or pathologically. (*Philadelphia Med. Times*, Jan. 13, 1883.)

The Antagonism of Opium and Nicotin.—Bonaccorsi gives the continuation of his studies on the antagonism of various remedies, which he began in 1877 with opium and belladonna. His experiments were made on rabbits, guinea-pigs, and frogs. From the results obtained with morphia and nicotin,

together and separately, and with morphia and hyoscyamin, morphia and aconitin, and morphia and daturin, he arrives at the following conclusions. (1) The antagonism between morphia and nicotin is a demonstrated fact. (2) There is no antagonism between morphia and aconitin, hyoscyamin, or daturin. (3) Opium and morphia act particularly on the cortical part of the brain, and on the arachnoid, producing hyperæmia, congestion, paralysis, especially of the vasomotor and of the respiratory centres. (4) Nicotin acts particularly on the brain and medulla oblongata, at first irritating it, and in prolonged action paralysing the nerves which arise from it. (5) Neither morphia nor nicotin has any special action on the blood, liver, kidneys, or bladder. (6) Opium has a depressing action on the splanchnic nerve, while nicotin excites the intestinal ganglia. (7) Death with morphia and nicotin is by asphyxia, with morphia from its paralysing action, with nicotin by its tetanising action on the centre of circulation. (8) In poisoning with nicotin, opium or morphia is to be preferred to any other remedy. (*Archiv. Med. Ital.*, Fasc. iii. and iv., 1882 ; *Med. Record.*)

Treatment of Infantile Paralysis.—Dr. W. H. Barlow writes to protest against the continued application of the term "infantile paralysis" to a disease not confined to infants, and suggests that the disease should be called "regressive paralysis." In referring to the great value of heat in the treatment of this disease, Dr. Barlow maintains that without resort to electricity one cannot combat fairly the disease ; the only way to stimulate the muscles to act is by means first of the continuous current and, later on, the induced ; and the earlier you commence the use of electricity the better, for after the lapse of two years it is of comparatively little effect. As next in value to electricity Dr. Barlow insists on the patient trying to move the limb himself, then to use passive movements and regulated gymnastic exercises. (*Brit. Med. Jour.*, Dec. 1882, p. 1248.)

Intestinal Concretions.—Dr. Schuberg considers that the chief seat of intestinal concretions is the cæcum. They may occur also in the colon. They are of two kinds, either regular fæcal calculi or concretions. The latter are consequent upon the former, with the exception of those due to the copious use of grain or of magnesia, &c. All other concretions are fæcal calculi impregnated with inorganic salts, and in the centre fæcal residues or foreign bodies can almost invariably be found. Foreign bodies generally form the starting-point of concretions ; the most common are the seeds of fruit and hairs. In man it is probable that hair often gets into the intestinal canal from the habit of biting the beard, and this is perhaps the cause why eighty per

cent. of intestinal calculi occur in man and only twenty in women. (*Virchow's Archiv*, vol. xc., p. 73.)

Influence of Peptones and Inorganic Salts on the Saliva.—A number of experiments on this subject have been made by Chittenden and Ely, which show that the activity of saliva is increased by peptones. The presence of 1·2 per cent. of peptone caused an increased conversion of starch into sugar amounting on an average to four per cent., and a similar result is obtained with glycogen. When the quantity of peptones is doubled, no further increase is observed in the digestive power of the saliva. The stimulating action of peptones is not due to the inorganic salts contained in them, for these, when used alone, have rather a retarding influence on the digestion. Dilute acids diminish or destroy the diastatic action of saliva, and the gastric juice, containing as it does dilute acid, usually has this effect. When peptones, however, are present in a digestive mixture to the extent of only one per cent., and the acid is of the strength of 0·025 per cent., they not only counteract the retarding influence of the acid but increase the conversion of the starch to the extent of seven per cent. above that effected by the saliva alone. Increasing the amount of peptones to two per cent. does not appear to have any effect whatever. In acid solutions of increased strength the peptones appear to exercise but slight if any influence; thus when the solution contains 0·1 per cent. hydrochloric acid, no diastatic action at all is observed, even in the presence of peptones. Control experiments with saliva, peptones, and acid without starch, yielded negative results, thus showing that the saliva is entirely without action on the peptones in an acid solution of the above strength. Again, digestions of starch, peptones, and acid without saliva also gave negative results, apparently proving that the peptones are wholly without diastatic action in the acid fluid. Sodium chloride and phosphate when present to the extent of 0·012 per cent. exercise but a very slight influence on the action of the saliva. Calcium phosphate, however, is seen decidedly to increase the diastatic action, though the amount contained in the peptones can have but little influence on the formation of sugar. Very striking is the great increase of sugar formed in the presence of twenty-four milligrammes of calcium phosphate. This result is certainly suggestive of a possible utility of the calcium phosphate invariably present in wheat, oats, and carbohydrate matter in general. What may be the exact explanation of the action of the phosphate in such a weak acid solution is of course hypothetical. The salt dissolves, in part at least, in the acid, and is thus probably modified with formation of bodies more favourable to the action of the ferment; it certainly cannot diminish to any

extent the strength of the acid. The inorganic matters contained in the peptones have little or no action, for in the sample containing the largest percentage of ash, and presumably therefore of calcium phosphate, and under the same conditions as this salt appears to work to the best advantage, only a slight increase is noticed in the diastatic action of the saliva. While the addition of 50 cc. 0·05 per cent. hydrochloric acid to an equal volume of saliva and water brings the diastatic action down almost to zero, the addition of one gramme of peptone to this acid fluid increases the diastatic action of the saliva to above its normal amount. In other words, the presence of one per cent. of gastric peptones in a salivary mixture containing 0·025 per cent. of acid, acts as a stimulant to the diastatic ferment, causing it to act with greatly-increased vigour. This fact appears to the writers to be of prime importance in solving the question as to the possibility of a continued action of the salivary ferment in the stomach. The salivary ferment is ultimately destroyed in the stomach by the gastric juice, but this fact does not necessarily preclude the possibility of a continued action of the saliva in the stomach. There is a growing impression that in the first stage of gastric digestion there is but little or no free acid present; thus Reinhard v. d. Velden claims, as a result of observation, and his results have been confirmed by Eedinger, that for the first three-quarters of an hour after eating a hearty meal human gastric juice fails to contain much, if any, free acid. This experiment makes it very apparent that by far the greater part of the starch is almost immediately converted into sugar. It seems therefore quite possible, especially in view of the action of peptones in a weak acid solution, that in the case of human beings there may be a continuation of salivary digestion in the stomach, provided the contents of the stomach during the first stage of digestion do not contain more than 0·025 per cent. of free acid. In a solution containing either 0·3 per cent. or 0·15 per cent. of sodium carbonate, the presence of the peptones nearly doubles (quite and even more in the case of glycogen) the diastatic action, bringing it up almost to the action of the unaltered saliva. Salts have little or no influence on diastatic action in alkaline solutions of the strength used. (*Pharm. Journal*, Nos. 645, 646, and 648.)

It is well known that when food is taken the contents of the stomach very soon show an acid reaction when tested with litmus paper, but only after 20″ to 60″ show an acid reaction when tested with tropaeolin or methyl-violet; *i.e. free* acid takes some little time to make its appearance. Since gastric juice is secreted during the whole of this time, and contains ·15 to ·5 per cent. of hydrochloric acid, it is not clear why free acid cannot be detected earlier. It has been shown by Danilowsky that

peptone and certain other proteids combine with acids, and Mr. Langley and Miss Eves of Cambridge explain the absence of free acid in the first period of digestion by supposing that certain of the proteids of the food combine with the acid of the gastric juice as it is secreted. They find further that proteids which are at all near the point of saturation with hydrochloric acid will completely prevent saliva from acting on starch, hence the assumption that saliva can act on starch in the stomach as long as no free acid is present requires revision ; if any considerable quantity of proteid capable of combining with acid is present, the conversion of starch to sugar will cease before free acid can be detected. Other things being equal, the greater the amount in the food of proteid capable of combining with acid the more sugar will be formed from starch in the stomach ; since milk and the white of unboiled eggs contain at least one body—globulin—which combines with acid, it follows that the addition of either of these to starchy foods facilitates the gastric digestion of these foods. This is perhaps of special importance when from any cause, such as an inflammatory condition of the small intestine, the pancreatic digestion is impaired. Certain other conclusions are arrived at which we may briefly summarise.

The action of ptyalin is just appreciably retarded by the presence of ·0015 per cent. of alkaline salt (Na_2CO_3) or of free acid (HCl), and in accordance with this the action of saliva is increased by neutralising. When saliva or other ptyalin-containing solution is as nearly as possible neutralised, the addition of peptone or of certain other proteids, the rate of conversion of starch to sugar is increased ; this effect of peptone in slightly alkaline and slightly acid solution has already been pointed out by Chittenden and Ely. When acid as well as proteid is added, the rate of conversion is decreased, but may still be quicker than in a neutral solution without proteid ; other things being equal, the rate depends upon the relative amounts of acid and proteid present. In other words, proteids which have combined with a small amount only of acid retard the action of ptyalin very slightly, the more nearly they are saturated with acid the greater their retarding action, and the presence of even less than 1 per cent. of proteid saturated with acid will completely prevent the conversion of starch to sugar by ptyalin. Peptone saturated with acid destroys ptyalin slowly ; if far from saturation-point it destroys ptyalin little or not at all. Alkaline salts, although retarding considerably amylolytic action, have very little destructive effect on the ferment ; as already shown by Chittenden and Ely, the retarding action is largely annulled by proteids.

These experiments confirm the previous conclusion arrived at by Mr. Langley, that all the ptyalin is destroyed in the stomach

(compare *Practitioner* xxviii. 453). It may be interesting to give the method of detecting free HCl with tropaeolin. Drops of saturated solution of tropaeolin (oo) in strong spirit are allowed to dry on a porcelain slab at 40° C. The slab is kept at 40° C. and a drop of the fluid to be tested is placed on the tropaeolin ; if free HCl (·006 per cent. and upwards) is present, a violet mark appears either at once or when the fluid evaporates. (*Cambridge Journal of Physiology*, vol. iv. No. 1, p. 18, 1883.)

Effect of Drugs on the Hæmoglobin of the Blood.—An investigation on this subject has led Fenoglio to the following results :—Preparations of iron have a very unequal action, and during their administration the quantity of hæmoglobin in the blood should always be tested. Lactate of iron and Bland's pills (consisting of oxide of iron and carbonate of potash) are preferable to Bravais' dialysed iron ; but though this preparation is not so powerful as the others, it is by no means without effect. The action of Fowler's solution becomes more powerful the longer it is continued. Notwithstanding the opposition of many authors, Fowler's solution is indicated in anæmia and chlorosis, and all conditions where the hæmoglobin of the blood is diminished, for this preparation both increases the hæmoglobin and improves the appetite and the general appearance. (*Wiener med. Jahrb.*, p. 636, 1882.)

Antiseptic Properties of Carbonic Acid. — Professor Kolbe has been engaged in endeavouring to find a method of applying the antiseptic action of salicylic acid to the preservation of meat. Experiments showed that acids in general, as well as their vapours, prevent decomposition. Carbonic acid acts in this respect like other acids, and is an excellent preservative for beef, which will retain its flavour in it for several weeks. Mutton acts quite differently, and after being kept in carbonic acid for a week begins to have a putrid smell. Veal does not keep so long as beef. Fish, oysters, and fruit only keep a short time. (*Pharmaceutical Journal*, Jan. 13, 1883.)

Action of Lead on the Stomach and Intestine.—Rudolf Maier draws the following conclusions from experiments which he made on this subject :—Rabbits and guinea-pigs died from lead-poisoning with doses of two decigrammes daily in from 10 to 266 days. In all of them marked changes were observed in the stomach and intestine. There occurred first, turbidity and fatty degeneration of the gland-cells ; second, dilatation of the arteries, venous congestion, hæmorrhages and circumscribed brown softening ; third, increase in the sub-mucous connective tissue, and sclerotic degeneration of the sub-mucous and mesenteric ganglia. The changes in these latter give rise to lead

colic. The changes in the intestine explain the emaciation of the animals. The author considers himself justified in drawing general conclusions regarding chronic lead-poisoning from his experiments, and in defining the condition as a parenchymatous degeneration with consequent induration of the connective tissue, similar to chronic phosphorus poisoning, and the symptoms are those of chronic hæmorrhagic inflammation, and of a neurosis affecting the most diverse parts of the nervous system. (*Virchow's Archiv*, p. 445, vol. xc.).

The Progressive Growth of the Dermoid Coat of the Membrana Tympani.—It has been recognised that there is a progressive growth outwards of the dermoid coat of the membrana tympani and lining of the external meatus, the constituent cells or scales not falling off *in situ* displaced by new cells from the deeper structures, as in other cuticular surfaces. This very remarkable provision for the removal of epidermic débris and adherent foreign matters may be the equivalent in cuticle of ciliary movement in mucous membranes, and the movement can be traced from the central portions of the membrane as far as the cartilaginous part of the external meatus, where the epidermis becomes much thickened, and the scales are finally shed. Dr. Blake has investigated the course of the epidermic elements on their way outwards by attaching minute discs of sized paper to different points of the surface of the drum, and mapping out their positions at intervals of a day or two. He has specially noted the course of the points on the central and more mobile portions of the membrane, and it may be generally stated that those behind and below the insertion of the malleus pass upwards and backwards towards the roof of the meatus, on lines converging to a point a few millimetres beyond the periphery of the drum. Even those placed in front of the tip of the malleus curve round so as to pass below and behind, not across it. Others higher in front pass directly upwards. The points have different rates of speed, the quicker being from points where the membrane is most mobile. On reaching the superior wall of the meatus they travel directly outwards, but with a tendency backwards, until they are finally detached. (*Otological Journal*, Oct. 1882.)

The Treatment of Deep Ulcers of the Cornea.—Dr. Guaita considers that the most severe forms of keratitic ulcers are of septic origin, though cases may be occasionally met with which develop spontaneously and without the contact of any septic secretion derived from the conjunctiva or lacrymal passages. Even then, however, predisposing causes favouring the invasion of septic agents are usually to be found. He is hence strongly disposed to recommend the treatment of such ulcers by anti-

septic measures, and advises that these should not be applied in a merely perfunctory manner, but carefully and skilfully, with proper precautions. He has himself experimented with carbolic acid, thymol, and with boracic and salicylic acids. The carbolic acid he has used in the form of spray in strength varying from one to five per cent., or in solution in strength of two to three per cent., and he applied carbolised gauze and impervious cloth over the eye. He found, however, that this strength irritated, and he replaced it with advantage by salicylic acid and by boracic acid, both of which proved much less stimulating. The former, when used as spray, excites no irritation with 0·3 per cent. solutions, though its use must not be continued too long, as it is apt to irritate the throat. Boracic acid may be used in the strength of six per cent. as spray. When there is a tendency on the part of the ulcer to penetrate into the anterior chamber, he employs the following ointment, which should be inserted between the lids once a day:—Neutral sulphate of eserine, one grain; boracic acid, half a grain; vaseline, 100 grains. (*Recueil d'Ophthalmologie*, March, 1883.)

Premature Delivery for the Prevention of Blindness.—It has long been known that pregnant women, especially towards the end of gestation, are liable to suffer from a disturbance of vision, which may vary from the slightest deterioration to a total and permanent blindness. Dr. Loring expresses a strong opinion that premature delivery may in some instances be justifiable for the restoration or preservation of sight. He cites various cases in support of this view. He remarks that very little care is bestowed by the general practitioner upon the condition of the urine in pregnant women, unless his attention is particularly called to it by some development in the symptoms, or some derangement in function. In the larger medical centres some very careful physicians do make an examination of the condition of the kidneys from time to time, even if there are no manifestations that make it urgent; and if albumen is found, a strict watch is kept upon the condition of the eyes. It is found that the retinitis subsides after the birth of the child. But it often happens that no complaint is made during pregnancy, and yet retinitis may exist. Loss of vision may first be noticed some months after confinement, and may then proceed to partial or total blindness. Since the affection of the eye is usually associated with albuminuria, and this again is eminently likely to lead to convulsions, Dr. Loring concludes that it is not only justifiable, but at times absolutely necessary, when a permanent loss of vision has occurred from a preceding pregnancy, that premature delivery should be induced in a subsequent one; and that, further, when loss of

vision, either temporary or permanent, has once resulted from gestation, it is the duty of the family physician to explain to both the wife and husband that the cause of the trouble is a constitutional and not a local one, and that there is every probability of recurrence of the trouble in succeeding pregnancies, which may lead, not only to the abolition of vision, but even to loss of life. (*Transactions of the American Ophthalmological Society*, 1882.)

Permanganate of Potash Pills.—For the preparation of these pills, recommended by Ringer and Murrell (*Practitioner*, xxx. 128) for amenorrhœa, Mr. Martindale recommends:—Vaseline two parts; paraffin wax one part: melt; stir till cold, and add kaolin, three parts; mix well. This binds the powdered permanganate together, and with a little dexterity the pills may be rolled out without much difficulty and dusted over with kaolin. They may be coated with sandarach dissolved in absolute alcohol and rendered tasteless. Cacao butter may be used as an excipient, but it in time reduces the permanganate, and it is troublesome to manipulate. In solution, a dose of permanganate is very nauseous. In a tasteless pill it dissolves slowly, and yet can be easily disintegrated; it is, besides, more agreeable to the stomach than in solution. (*Pharmaceutical Journal*, Jan. 13, 1883.)

Renal Inadequacy.—Under the head of Renal Inadequacy Dr. Andrew Clark places a class of cases, which in some measure he ventures to separate entirely from Bright's disease, in which the kidney, without any sensible alteration of structure that modern means of investigation will enable us to determine, cannot produce a healthy urine. Such kidneys produce a urine which, assuming the quantity to be the quantity of health, is low in density, and is deficient in solid constituents, principally the constituents of urea and its congeners. This condition may be regarded as a very early stage of Bright's disease, but the writer thinks it of practical value to recognise by a distinct name a state which may remain as it is during the whole period of life, which is nevertheless capable of removal, and which if unnoticed may lead to serious injury to the patient.

Such patients are characterised by three things particularly: (1) By a curious inability properly to repair damages done to them either by accident or by disease. (2) They not only repair damages slowly, but are peculiarly vulnerable; they catch cold, for example, easily, and get rid of it with difficulty. (3) One can never be sure of the result of the performance of an ordinary surgical operation upon them; they die from a simple operation by hæmorrhage, or have an ordinary abscess opened and become pyæmic.

Dr. Clark confesses that he knows of no symptoms in the early stage whereby these cases can with certainty be detected, but says in general, " When you get hold of a patient who is ill, suffering from dyspepsia or nervousness, having headaches, and complaining of malaise and weakness, who cannot sleep well, who cannot do his work very well, examine his urine, and if you find that the urine is low in density you had better proceed a little further, and be very precise, and get the urine of twenty-four hours, and if you find that it is under fifty ounces in quantity, that it has not a specific gravity of 1010, and that the urea in it is deficient in amount—under two per cent.—then whether there be albumen in the urine or not, whether there be any casts or not, whether there be granular débris deposited or not, you may know with certainty that the kidney is not doing its duty."

It may not be that this defect is the cause of the patient's ill health, but if it is found in addition that an increase of food makes the patient worse, and that, within certain limits, a diminution of food makes him better, there need be little doubt of it. As these cases of renal inadequacy get worse if they be not well managed, that is, if the management be not adjusted to the fact that they are like chimneys being choked, so that a roaring fire cannot be kept on, they develop exceedingly characteristic symptoms, symptoms so like those observed in myxœdema that they can scarcely be distinguished. Dr. Clark cites two autopsies on patients who presented the symptoms of renal inadequacy, one in the earlier, and one in the later stage, in which none of the gross appearances of renal disease were present.

In the way of prognosis it may be said in regard to these cases that if care be taken, if compensation be made for the defective kidney, they probably may go on indefinitely, that is, they may reach the full term of life allotted to man.

Treatment should consist in close attention to simple physiological laws and careful adherence to such rules in regard to diet, clothing, temperature, and exercise as are generally laid down for chronic albuminuria. (*Brit. Med. Journ*, Feb. 24, 1883.)

The Vomiting of Pregnancy.—Mr. Brock discards the numerous theories which have been proposed to account for obstinate vomiting in pregnancy, and believes that it arises simply and purely from an idiosyncrasy in the individual. Vomiting, of course, may be aggravated by other conditions present, such as undigested matters in the alimentary canal, etc.

He thus summarises his principal reasons for coming to this conclusion. (1) That obstinate vomiting occurs in multiparæ, where the uterine tissues are lax, and where the os is soft, easily dilatable, and even patent enough to admit the tips of two

fingers. This causes him to reject the theory held by Bretonneau and Barnes. (2) That obstinate vomiting is absent in the majority of cases where there is a rigid state of the os, and where one would almost expect it invariably to be present, if the cause were that assigned by Dr. Barnes. (3) That obstinate vomiting is often absent in flexions and distortions of the uterus, and often present where there are no flexions or distortions. This would not be likely if Dr. Hewitt's theory were true. (4) Obstinate vomiting is often absent in inflammatory conditions of the uterus, and present when there are no inflammatory conditions. This ought not to be the case if Dr. Bennett's theory be correct. (5) Because he believes a parallel condition is to be seen in other affections clearly influenced by the individual's neurotic constitution; for instance, obstinate sea-sickness, the occasional vomiting that takes place in spurious pregnancy, the proneness to convulsions in certain children whenever ill; or, to take a specific case, the vomiting simulating the obstinate vomiting of pregnancy in a non-pregnant woman in whom the uterus was normal. (6) Because there is no definite line to be drawn between the ordinary cases of sickness in pregnancy and the more severe cases. (*Glasgow Med. Journ.* March, 1883.)

The Contagion of Measles.—Dr. A. Beclère has made a careful study of the conditions attending the contagion of measles, from which we select his more important conclusions.

Rubeola is contagious from the commencement of the period of invasion to the end of the stage of eruption, a period extending through from eight to ten days. The contagious principle is contained in the secretion of the respiratory mucous membrane, and it still remains to be proved that it has anything to do with the cutaneous desquamation. Although this contagion is diffusible, it is so only to a slight extent, and soon loses its active properties, and does not remain in the rooms occupied by the sick. The period of incubation lasts from thirteen to fifteen days; no immunity is conferred by the presence of any other eruptive disease. (*Gaz. Méd. de Paris,* Feb. 17, 1883.)

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Department of Public Health.

CHANNELS FOR CONVEYING INFECTION TO HOUSEHOLDS: HOW THEY MAY BE RECOGNISED AND DEALT WITH.

BY R. THORNE THORNE, M.B. LOND., F.R.C.P.

(*A Lecture delivered at Cheltenham on March 15th*, 1883.)

(*Continued from* p. 393.)

II. The second point to which I desire to draw your attention in connection with the subject I have chosen for this lecture has reference to intermitting water supplies. Our public water services are of two sorts. Firstly, there is the so-called "constant service system," and secondly, there is the "intermittent system." Water is delivered to consumers continuously under constant pressure; or else intermittently, either directly through service-pipes or by means of cisterns, the mains being fully charged at one time and either partially or entirely empty at another.

For many years before 1873 there had been certain warnings as to the evils attendant upon the intermitting as opposed to the constant system of water-supply, but none of these warnings, so far as I can ascertain, had indicated precisely in what the danger consisted. The credit of specifying at least one of the main sources of danger belongs to my colleague, Dr. Blaxall, who, during an inquiry into an outbreak of enteric fever at Sherborne early in 1873, found that, owing to the intermissions in the water-service, foul matters had facilities for making their way into the water-mains. Later on in that year a serious and fatal outbreak of the same disease occurred in Caius College, Cambridge.[1]

[1] See *Report of the Medical Officer of the Privy Council and Local Government Board.* New Series, No. II. 1874.

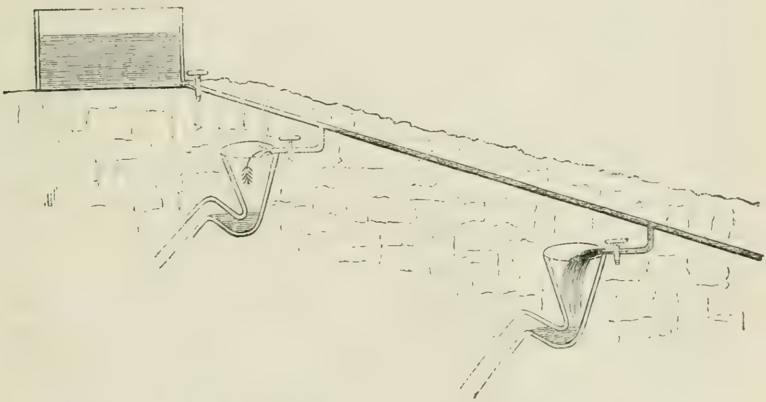
The circumstances were inquired into by Dr. Buchanan, F.R.S., and it was there conclusively shown that the spread of the disease had mainly been due to the suction of a specifically contaminated air into the water-pipes of a certain portion of the college during periods of intermission in the supply, and to the subsequent mingling of this air with the water which was used for drinking purposes. In the following year I myself received instructions to inquire into an outbreak of enteric fever in Lewes, and a brief reference to the facts which I there elicited may suffice to indicate to you how various are the ways in which a water which at its source is pure and wholesome may, owing to intermissions in the course of its supply, become contaminated, and hence serve as a channel for conveying infection to those households to which it is delivered.

Lewes, as you are aware, is a small town in Sussex; it is in the main situated on the ridge and sloping sides of a spur of chalk running from the neighbouring South Downs into the valley of the Ouse, a small portion also lying at the foot of the spur on a bed of river gravel. At the date of my inquiry the population was some 11,000. Towards the end of July and the beginning of August enteric fever broke out with considerable violence in the town, and by the end of August sixty-nine persons had already been attacked. A slight lull then ensued, only however, to be followed by a large accession of attacks, and in the five weeks ending October 10th, seventy-nine more patients had been stricken down. Again there came a marked and sudden burst, 104 fresh attacks occurring in one week, and these were followed by 156 more in the next fortnight. Somewhat early in the course of my investigation I had ascertained that, although the number of houses supplied from the mains of the public water-service was smaller than that of those having supplies from local wells, yet the incidence of the disease had almost without exception been upon those who used the town-water, and during the earlier portion of the epidemic I found the disease had picked out from amongst families of habitual well-water drinkers those members who had had opportunity of using the town-water. I also ascertained that the spread of the disease could not be attributed to the water as derived from its source in the springs

at the foot of the chalk range. But I learned that the supply was an intermitting one. Having regard to these considerations, and to the experience of Dr. Blaxall and Dr. Buchanan, and being also much impressed with the sudden occurrences of disease amongst the town-water drinkers, I made inquiry as to the existence of any of those conditions which I then knew had elsewhere been associated with the spread of enteric fever in places having intermittent water-services. None were known of, and I was confidently assured that none existed. Provisionally accepting the assurance, I was led again to review the early history of the outbreak and to consider whether the cause of the epidemic might not after all be found elsewhere than in the conditions of the water service. During my further search, however, I came across an instance of one of the very conditions which I knew had, in connection with intermissions in a water-service, led to accidental pollution of the public supply, and this first discovery was soon followed by others of a similar character.

The Lewes water-supply was derived from springs situated in the river-valley to the south-east of the town. From the water-works it was pumped into three service-reservoirs, situated at a considerable elevation on the chalk range, and from these again it was delivered to the town by means of the public water-mains. The supply from the reservoirs was, with certain exceptions, an intermitting one, the mains only being kept charged from 6 o'clock to 10 o'clock in the morning, and again from 3 o'clock to 6 o'clock in the afternoon. Under these circumstances it will be obvious to you that every morning and evening, after the water was turned off at the reservoirs, the contents of the mains would continue to be drawn away, but no fresh supply would replenish them for many hours. This was known to the public, and hence large quantities were some times stored by them so that they might have a supply during the intermissions, and on the other hand certain water-taps were allowed to remain always open, so that for the few hours during which the mains were charged, ample means for flushing drains, water-closets, and other filth-receptacles might be obtained. In this way it came to pass that in the course of the mains a number of openings always

existed at different levels. From the lower of these openings water would flow; through the upper ones, air or any other matters present at the orifice, would be drawn in. (See Diagram.) That such suction-action from without into the mains did actually take place was undoubted; for the flame of a lighted candle placed at the mouth of a tap, which was found open during a period of intermission in the water-supply, was immediately sucked up and extinguished, and the body of air thus drawn into the water-pipes was at times such that when the mains were re-charged from the reservoir, it was expelled at certain taps by a loud blowing noise, and the first water drawn



was found to be so charged with air that it was full of minute bubbles "like soda-water."

If the air thus daily drawn into these mains to replace the water as it was drawn off had always been pure, no harm could have resulted, but at many points in the course both of the mains and of the service-pipes it was found that foul air and other matter could be drawn into them, and not only so, but that an air laden with the specific infection of enteric fever was first sucked into these pipes, and subsequently distributed with the water to the inhabitants. Thus, at different elevations in the town, service-pipes were laid direct from the mains into the pans of water-closets without the intervention of any cistern. Whenever, during an intermission in the water-supply, a tap

fitted to such a service-pipe was opened at a point above the level of the water in the mains, foul air would at once be forcibly sucked into them, and several instances came under notice in which the air so drawn in must necessarily have been contaminated with material containing the specific poison of enteric fever. In a similar way I found that filth was drawn into the mains through holes in the service-pipes. Nearly all these branch-pipes were of lead, and wherever pipes made of this material had remained for any length of time in a soil which was contaminated by sewage matter, it was found that they rapidly became "worm-eaten" and destroyed, and during the intermissions in the service the filth by which they were surrounded, was sucked into the mains. (A sample of one of these perforated pipes was here exhibited.) One instance which was brought under my notice illustrates this danger in a very forcible manner. A householder noticed that on several occasions when the water-mains were re-charged, ordinary coal-gas was expelled from his water-tap in advance of the water, and it became evident that at some point or other a leaky gas-pipe and a damaged water-pipe were so near together that on the occurrence of the intermissions in the water-service the gas was forcibly sucked into the water-pipe. With a view of ascertaining the locality of the damaged pipes, the course of the water-pipe was opened up down his garden, until at last it was found to pass beneath an ashpit. The faulty portion of the pipe not having yet been reached, it became necessary to trace it still further, and an excavation was made beneath this receptacle for filth. At this point the lead service-pipe was seen to be in part destroyed. Now it so happened that the ashpit held sloppy contents, and that in the course of the work carried out, the liquid filth came into immediate contact with the perforated water-pipe. It also happened that this part of the work was carried out during a period of intermission in the water-service, and there being a partial vacuum in the mains, a loud suction-noise was suddenly heard, and the liquid contents of the ashpit disappeared up the water-main.

Again, during the course of my inquiry, I heard it asserted that at several points, lead service-pipes passed through drains, and it was also stated that at one spot the water-main passed

through the centre of one of the public sewers. This sewer was in consequence opened up, and when the arch of the culvert was removed at the point where the water-main passed through it, a jet of water suddenly shot up into the air. There was a hole in that portion of the water-main which was inside the sewer, and the main being at the time fully charged, the water was forced through it. But what took place during the intermissions? It is evident that at every intermission, as the water was abstracted from this portion of the main, sewer-air, if not sewage itself, was drawn into it, to be mingled with the water before it was again distributed to the town.

The sudden outbursts of disease which characterised this epidemic are now easy of explanation. There had for years past been numerous opportunities for the admixture with the town-water of foul air and other matters, but enteric fever once imported, the specific material of that disease itself then became from time to time mingled with it, and was consumed by the inhabitants.

I have already explained that at an early stage of my inquiry I was convinced that the disease was being distributed by means of the public water-service, but it was not until I was able to point out to the directors of the water company that facilities actually existed for the spread of the epidemic by reason of the intermissions in their water-service, that I could point to the remedy. The moment this discovery was made, the remedy was however obvious. It was that a constant, instead of an intermittent service should be provided for the town. Fortunately the directors were by this time as fully impressed with the importance of this step as I was myself, and as soon as the machinery at the pumping-station could be fitted for the extra work, a constant service was provided, the mains were kept permanently charged under a high-pressure service, and although leakages were numerous, and much waste resulted, yet no further suction into the mains could take place. This measure was adopted on the 13th of November, but in view of the fact that by this date there had already occurred some 450 attacks of enteric fever, and that almost numberless centres of infection were then in existence, I could hardly anticipate any instantaneous cessation of the epidemic. The result, however,

surpassed my most sanguine expectations, for whereas in the four weeks ending November 14th, as many as 198 fresh cases of fever were recorded, only 19 fresh attacks took place in the four weeks following on the usual interval of incubation after the constant service became general. The epidemic indeed, as such, was at an end.

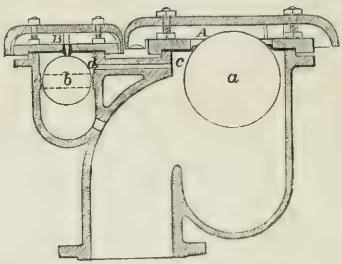
You may perhaps think, that I have been pointing to some very exceptional risks in connection with intermittent water-services. I would assure you that this is by no means the case. Wherever such services exist, the risks are the same in kind as those obtaining at Lewes, and the only exceptional features recorded have reference to the quality of the contaminating material which, under the circumstances detailed, found its way into the mains, and to some of the methods by which it gained access to them. In London we have an intermittent water-supply, and though the fully-charged state of the principal mains, together with the general provision of service-cisterns, removes some of the dangers of aërial pollution which prevailed at Lewes, yet there can be no question, but that wherever there is a defective water-main or service-pipe either uncharged or only partially charged, at that point there is danger that foul air, or other foreign matters may be drawn into them and become mingled with the water. Not long since, a case was reported to me in which coal-gas was expelled from the ball-tap in connection with a London cistern in such quantity as to lead to an explosion; the explanation being the same as in the parallel case at Lewes, namely that at some point a leaky water-pipe and a leaky gas-pipe were in such proximity to each other, that during an intermission in the water-service, the coal-gas which had escaped into the surrounding soil was sucked up into the water-pipe, and afterwards forced out when the water was turned on at the mains.

The Rivers Pollution Commissioners in their *Report on the Domestic Water-supply of Great Britain* strongly urge the abolition of intermitting water-services. They allege many grounds for this recommendation. Where a constant service exists, the supply can always be obtained cold and pure, straight from the mains. Where an intermittent system prevails, the supply must almost necessarily be stored. In the courts and

yards of our cities, the position of many of the cisterns should of itself suffice to condemn an arrangement involving the storage of water intended for domestic purposes, and even in our better houses it is by no means an uncommon thing to find cisterns near ash-pits, or in the space under roofs into which the hot air of the dwelling rises. And even when they are free from objection in point of situation, cisterns as a rule contain a deposit of dirt, their contents are rarely, if ever, completely changed, and the stored water becomes vapid and unwholesome. But to revert to the one special danger which I have been pointing out to-night, I would quote one item of evidence which was laid before the Commission by Dr. Thomas Davidson of Berwick-on-Tweed. In that town the water-service was a constant one during nine months of the year, but for the remaining three months the mains were only kept charged for about fourteen hours out of the twenty-four. During the intermissions, matter of the foulest description was sucked up into the mains, and in describing some of the materials thus found to mingle with the water the evidence states that "regurgitations . . . . of blood from slaughter-houses had . . . . passed into the street mains."

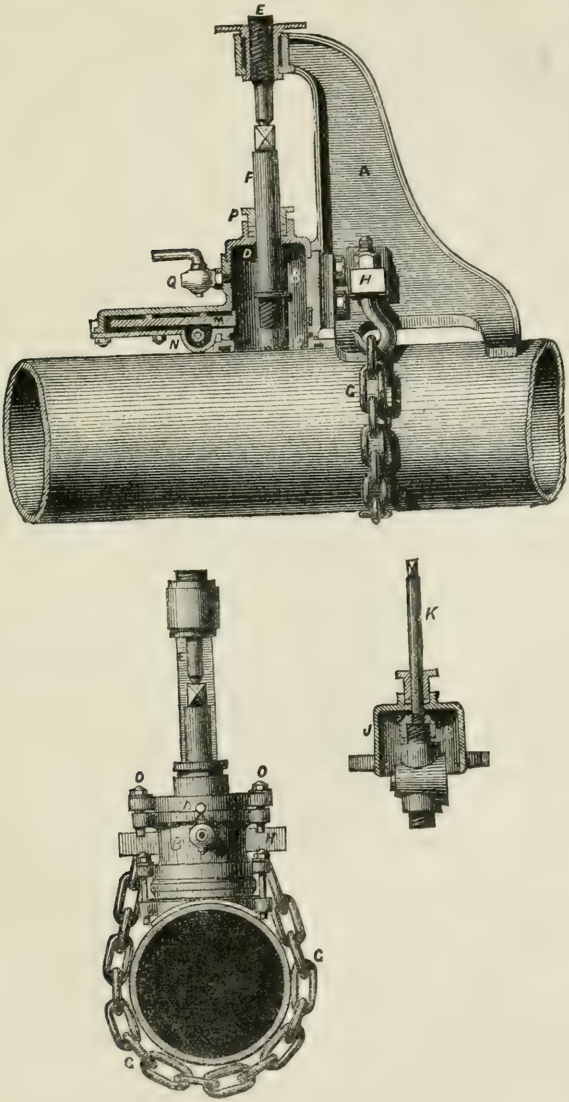
I trust I have now said enough to show you how great are the risks attendant on an intermitting water-service, and to convince you that unless all organic impurity is rigidly excluded from our water-supplies we can never be certain that foreign matter of a morbific quality may not be conveyed into our households through the channel of an intermitting service. There are, however, occasions when air from without must almost necessarily be admitted into our water-mains, and with a view to such circumstances engineers have contrived several ingenious inventions. Air must be admitted into the mains whenever these are emptied either in whole or in part during certain processes of repair, and also on the occasion of accidental intermissions which may occur in services professedly constant. Air-valves have hence been designed with a view of insuring the purity of any air that is admitted into the mains, and of providing for the escape of air from any which, having been emptied, are again being charged with water. The annexed diagram shows an air-valve manufactured by Messrs. Tylor and Son, and it indicates the principles to be held

in view in their construction. The water-main being fully charged, a wooden ball *a* coated with a preparation of vulcanite is floated up, so as to close the aperture in the upper part of the chamber of the valve, and it is there retained by the pressure of the water. Directly the pressure from below is taken away, as on the occurrence of an intermission in the service, the ball descends with the falling water until it is deposited in the curved hollow provided for it, fresh air being at the same time freely admitted from above. But a further object is served by this ingenious apparatus, which has two valve-chambers. On the re-charging of a main which has been emptied, the main body of air to be expelled is forced through the two apertures A and B until the water rises high enough to float the two balls *a* and *b* against the apertures which they respectively close. After a



time, however, air-bubbles are rolled along the inside of the upper surface of the main, and on reaching the air-valve they accumulate in the highest part of the two chambers, passing from the larger into the smaller one by means of the aperture *c d*. The ball *b* is so weighted as to remain floating so long as it is rather more than half immersed in the water. The natural tendency of this ball is to fall, but this tendency is resisted by a slight excess of upward pressure, which is due to the fact that a small portion of the ball at the opening B is only exposed to the atmospheric pressure instead of to the much greater pressure which obtains inside the air-valve. As soon as the water is lowered sufficiently to make the tendency to fall exceed this slight excess of upward pressure, the ball sinks, the aperture B is opened, and the air escapes; the level of the water then again rises and the ball is carried up to its original position; the same action being repeated so long as there

remain any small quantities of air to be passed out of the main. This action could not take place in the case of the



larger ball a, because even if the water were forced down below the ball, the weight of the ball would not be sufficient to over-

come the large excess of upward pressure, which is brought about by so large a portion of its surface at A being exposed to the smaller atmospheric pressure. Indeed, if the aperture B were made too large the action described as regards the ball *b* could not take place. Hence the value of the smaller chamber.

Localised intermissions in professedly constant water-services have perhaps been most frequent on occasions when new service-pipes are being fixed into water-mains, and hence with a view of preventing the intermissions and the consequent admission of air into the mains, certain methods have been devised for fixing such pipes whilst the mains remain fully charged and under pressure. Of the several inventions having this object in view "Upward's Patent Safety-drill for Water-mains" is specially ingenious, and it exemplifies how the needed requirements may best be carried out. The valve-chamber B, shown in the reduced diagram on page 479,[1] is in the first instance firmly clamped on to the main. The slide-valve M is then drawn back into the position shown in the diagram, by means of the pinion-spindle N, and the drill and tap having been placed over, and secured to, the valve-chamber, the drill-spindle is pressed down until the point of the drill touches the pipe. The drilling is then commenced, and as soon as the hole is through, the drill-spindle is pressed down until the tap-pipe bites, and the hole is tapped. The tap-pipe and drill being then withdrawn high up into the valve-chamber, the slide-valve M is carefully closed, the water in the chamber above it is allowed to escape by means of the cock Q, and the drill-spindle and its cover are removed. A cock-chamber with a service-tap screwed on the end of the spindle K is then attached to the valve-chamber B, the slide-valve M is opened, the water-cock is screwed into the main by means of the spindle, and the whole apparatus being removed, the cock remains securely fixed into the main which has from first to last remained fully charged.

[1] From a woodblock kindly lent by Messrs. Easton and Anderson, Engineers, 3, Whitehall Place, S.W.

(*To be continued.*)

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